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ARTICLES

American Political Science Review Vol. 89, No. 2 June 1995

BEYOND SES: A RESOURCE MODEL OF POLITICAL PARTICIPATION

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This paper develops a resource model of political participation. The resources considered are time, money, and civic skills—those communications and organizational capacities that are essential to political activity. These skills are not only acquired early in life but developed in the nonpolitical institutional settings of adult life: the workplace, organizations, and churches and synagogues. These resources are distributed differentially among groups defined by socioeconomic status. A two-stage least squares analysis shows these resources have powerful effects on overall political activity, thus explaining why socioeconomic status has traditionally been so powerful in predicting participation. We disaggregate overall activity into three kinds of acts: those that involve giving time, those that entail donating money, and voting. Each requires a different configuration of resources resulting in different patterns of stratification across various political acts.

Why do citizens participate in political life? One way to think about this puzzle is to invert the question and ask why people don't take part in politics. Three answers immediately suggest themselves: because they can't, because they don't want to, or because nobody asked.[1] "They can't" suggests a paucity of necessary *resources*: *time* to take part in political activity, *money* to make contributions, and *civic skills* (i.e., the communications and organizational skills that facilitate effective participation). "They don't want to" focuses on the absence of psychological *engagement* with politics—a lack of interest in politics, minimal concern with public issues, a sense that activity makes no difference, and no consciousness of membership in a group with shared political interests. "Nobody asked" implies isolation from the *recruitment networks* through which citizens are mobilized to politics.[2]

All three factors help explain political participation, but we focus on the role of resources—time, money, and civic skills[3]—for explaining political participation in America. Adding resources to the other two explanations permits us to move beyond the "SES model," that is, beyond explanations of political activity based on one or more of the components of *socioeconomic status*: education, income, and occupation.[4] By attending to resources conceived at a general level, we can probe the way resources link backward to SES and other social characteristics and forward to political activity. Going backward from resources, we can show that the three resources of money, time, and civic skills vary in their association with SES and other social characteristics. Money and some kinds of civic skills are closely related to SES, but time and other civic skills are less stratified. Civic skills are less stratified by SES partly because social characteristics such as affiliations with "congregational" churches are not highly correlated with SES and these affiliations serve as training grounds for civic skills. As we go forward from resources to political activity we can show how the importance of a resource depends upon the particular activity. Education, for example, is important for some political activities because it enhances political interest and civic skills while income is important for other activities because of the monetary resources it provides.[5] By showing how resources differentially available on the basis of SES affect various modes of political activity, we explain not only why some individuals are more active and others less but also why certain kinds of people engage in particular kinds of political activity.

A resource-based approach also has methodological and theoretical advantages, especially in comparison to explanations based solely on psychological engagement with politics, thus yielding a more powerful explanation of participation. We are more confident in our ability to measure resources than in our ability to measure psychological engagement. Reports of attitudes are notoriously fugitive, unreliable, and difficult to compare across respondents (Duncan 1984). As we shall see, the measurement of resources rests on more factual questions for which the metrics used—dollars, hours, and the number of letters written or speeches given—are unlikely to vary in meaning from respondent to respondent. Although responses may suffer from the fallibility of human memory, at least these questions are about concrete, everyday matters. Furthermore, when linking engagement and activity, it is hard to be certain of the direction of the causal arrow. Political interest and political efficacy, for example, certainly facilitate political activity, but activity presumably enhances interest and efficacy as well. Indeed, most measures of psychological engagement with politics are, by their very nature, perilously close to activity itself. This makes them robust predictors of political participation but trivial (and possibly spurious) explanations for participation. Yet despite its apparent "head start" as a predictor of participation, we show that political interest—a standard measure of psychological engagement in politics—does not displace resources as a predictor of political participation.

Theoretical considerations lend additional support to the notion that a resource model can provide a powerful explanation of political participation. Unlike psychological engagement in politics, which probably develops along with political activity, the institutional involvements from which citizens acquire resources generally antedate or occur independently of political activity. Obviously, family background and early experiences in school—critical for both the development of resources for politics and for the future institutional commitments that permit the further enhancement of political resources—precede adult activity. In the absence of actual life histories collected over respondents' lifetimes, we cannot be absolutely certain that adult decisions about family, work, organizational involvements, or affiliations with religious institutions are apart from and in advance of choices to take part politically, but these seem plausible assumptions.[6] In addition, a resource theory has implications for the normative issue of how we construe political inactivity, especially when the politically quiescent have obvious and pressing needs. If individuals eschew politics because they do not care—because they prefer to devote themselves to private rather than public pursuits—then we are apt to dismiss inactivity as a matter of personal choice. If the failure to get involved is the consequence of resource constraints that make it difficult for even those who are politically interested and engaged to take part in political life, then we are likely to be more concerned about political inactivity.

Finally, resource models of political participation tie into two powerful intellectual traditions: stratification theories from sociology and individual choice perspectives from economics. The SES model follows naturally from stratification theories,[7] which suggest that class and status hierarchies are fundamental features of modern industrial societies that often determine their politics. True to the theory, the SES model does an excellent job *predicting* political participation.[8] Yet the SES model fails to specify clearly the *mechanism* linking social statuses to activity. Rational choice theories, on the other hand, have clearly specified how and why individuals might decide to participate in politics to pursue their self-interest, but these theories have done a very poor job predicting political participation.[9] Indeed, with respect to SES and participation, at least one variant of the rational choice approach suggests no relationship at all or, if any, that people of high SES (who by virtue of their high levels of education command the intellectual sophistication to comprehend the free-rider problem and who by virtue of their high salaries would find the opportunity cost of participation prohibitive) to be least likely to take part in politics. Instead those with high levels of SES, who are not otherwise known for particular irrationality in the conduct of their lives, are the most likely to be active.[10]

The problem is that rational choice approaches have focused on how the benefits of participation might offset the costs of participation without examining costs very carefully. A resource perspective takes seriously the costs of using resources. In doing so, we apply to politics an important variant of rational choice theory. The Chicago school of economics[11] has shown how a powerful theory of choice can be built not upon restrictions on the motives for choice (as the emphasis on self-interest over altruism or duty does) but upon the budget constraints on resources that limit choices. If there are multiple constraints on a series of resources that vary independently in the population, then a formidable theory can be based upon the degree to which each resource constraint is binding in a particular situation. And since resources such as money, time, and skills can be measured and affected by policymakers, it is useful to formulate a theory based on a careful description of how variations in resources flowing from social stratification enable and restrict individual activity.

To develop a resource model of political participation requires four steps. First, we define resources and explicate how we measure them. Second, we show how resources are distributed in the population, in particular, how they relate to SES. Third, we look closely at the resource of civic skills (particularly those acquired as an adult) in order to show that our somewhat indirect indicators of civic skills indeed measure a single dimension of civic skill that is developed in extrapolitical institutional settings and available for political activity. The fourth step is the heart of our enterprise—we show that resources explain political participation and that different resources are related to different activities.

THE CITIZEN PARTICIPATION STUDY

Our data come from a large-scale, two-stage survey of the voluntary activity of the American public. The first stage consisted of over 15,000 telephone interviews of a random sample of the American public conducted during the last six months of 1989. These 20-minute screener interviews provided a profile of political and nonpolitical activity as well as basic demographic information. In the spring of 1990, much longer, in-person interviews were conducted with a subset of 2,517 of the original 15,000 respondents chosen so as to produce a disproportionate number of both activists as well as African-Americans and Latinos. In the following analyses, we have reweighted the follow-up sample so that we have a representative sample. (See Verba, Schlozman, Brady, and Nie 1993 for a description of the sample.)

The study is unusual in focusing on voluntary activity not only in politics but also in churches and organizations. In addition, we construed political participation quite broadly, including not only voting and other forms of electoral activity (e.g., working in campaigns, making financial contributions) but also contacting public officials, attending protests, and getting involved either formally or informally on local

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issues. (Appendix A provides a list of the questions used in this paper.)

DEFINING AND MEASURING POLITICAL RESOURCES

We begin by outlining the resources for political participation.

Time and Money

The two prime resources for investment in political participation are time and money. Individuals use time in the service of political action in many ways (e.g., working in a campaign, writing a letter to a public official, attending a community meeting). Money, of course, can be donated to candidates, parties, or innumerable political organizations or causes. We measure money resources by family income from all sources in $10,000 units.[12] We measure the "free" time available for political activity by the hours, if any, left over after accounting for time spent in an average day doing work for pay, doing necessary household work of all sorts, studying or going to school, and sleeping.[13] Time and money differ in significant ways as resources. In comparison with money, time is both more constrained and more equally distributed—everyone has only 24 hours in a day. The upper limit on money, of course, is much less constrained, and differences among individuals can be much larger. Time is constrained in another way that affects the way it is distributed. Time not used today cannot be put in the bank. Money, in contrast, can be accumulated for later use.[14]

Civic Skills

Civic skills—those communications and organizational capacities that are so essential to political activity—constitute a third resource for participation.[15] Citizens who can speak or write well or who are comfortable organizing and taking part in meetings are likely to be more effective when they get involved in politics. The acquisition of civic skills begins early in life—at home and, especially, in school. However, the process need not cease with the end of schooling but can continue throughout adulthood. Adult civic skills relevant for politics can be acquired and honed in the nonpolitical institutions of adult life—the workplace, voluntary associations, and churches. Managing a reception for new employees and addressing them about company benefits policy, coordinating the volunteers for the Heart Fund drive, or arranging the details for a tour by the church children's choir—all these undertakings represent opportunities in nonpolitical settings to learn, maintain, or improve civic skills.

We measured civic skills in several ways. Since communications and organizational skills are acquired in school, we asked an extensive set of questions about educational attainment.[16] From these we constructed an eight-point scale ranging from a grammar school education to a Ph.D. or professional degree.[17] In addition, one particular aspect of educational experience—participation in student government in high school—provides a potentially useful measure of civic skills.[18] We measure this by a four-point scale ranging from no activity to very active. Since facility in expression is central to the ability to communicate effectively, we also use two measures of language ability, testing somewhat different notions of how verbal facility matters. American society puts a premium on speaking and understanding English, so we asked our respondents what language they ordinarily speak at home: English (scored as 3), a combination of English and another language (2), or another language (1). We thought that those who did not speak English at home would be less likely to find it easy to participate in politics. In addition, to assess verbal ability, we used the score (number of items correct) on a 10-item vocabulary test that has been used regularly since 1974 on the National Opinion Research Center's General Social Survey.[19] For the 1% of our sample interviewed in Spanish, this test was administered using Spanish words.[20] Alwin notes that this vocabulary score is strongly related to schooling (a correlation of .51 in our sample and .54 in Alwin's), but such scores also "correlate highly with tests of general intelligence—usually .8 or higher—and are good indicators of scores on the verbal component of standard tests of general intelligence" (1991, 627).[21] Years of education and vocabulary score are included in our model to show that both schooling and general intelligence matter for political participation and to provide additional support for our contention that civic skills matter when other factors are controlled.

To measure civic skills developed as an adult, we asked those with jobs and those who reported activity in a church or an organization[22] whether as part of their involvement in each sphere, they had, in the past six months, engaged in the following activities: written a letter, gone to a meeting where decisions were made, planned or chaired a meeting, or given a presentation or speech. In each realm (on the job, at church, or in nonpolitical voluntary organizations) we measured civic skills as the number, ranging from zero to four, of these *skill-acts* undertaken by the respondent in the last six months. Those who have an opportunity to do these things in a nonpolitical setting would, presumably, be more willing and able to do them in a political context. In this sense, we expect that these competencies can be used as independent variables to explain political participation. Table 1 presents, both for all respondents and for only those involved in each arena, the frequency with which individuals engage in activities that we expect to produce civic skills. People are most likely to engage in skill-acts at work: 53% of the sample practiced at least one skill on the job in the six months before the survey. Yet 33% of the population engaged in skill-acts in nonpolitical organizations and 20% in churches or synagogues.

TABLE 1

Percent Reporting Various Activities in Nonpolitical Settings

CIVIL SKILL	ON THE JOB: % OF WHOLE SAMPLE	ON THE JOB: % OF WORKING[a]	IN NONPOLITICAL ORGANIZATIONS: % OF WHOLE SAMPLE	IN NONPOLITICAL ORGANIZATIONS: % OF ORGANIZATIONALLY INVOLVED[b]	IN CHURCH OR SYNAGOGUE: % OF WHOLE SAMPLE	IN CHURCH OR SYNAGOGUE: % OF CHURCH MEMBERS[c]
Attend a meeting where decisions are made	48	70	30	41	18	27
Plan such a meeting	24	36	14	19	9	13
Write a letter	40	58	15	16	6	9
Make a speech or presentation	28	40	15	18	10	15
At least one of the above	53	78	33	44	20	29

Source: Citizen Participation Study.

[a] Working full- or part-time; with a job but not at work due to vacation, illness, etc.

[b] Member of or contributor to an organization that does not take stands on public issues.

[c] Member of or regular attender of services at a local church or synagogue.

THE DISTRIBUTION OF POLITICAL RESOURCES

As we shall demonstrate, the presence or absence of resources contributes substantially to individual differences in participation. Resources are, in turn, not equally distributed; some socioeconomic groups are better endowed than others. This makes a resource model useful not only for explaining individual differences in political activity but for explaining differences in activity among politically significant social groups, especially differences along SES lines. However, resources vary in the extent to which they are stratified by SES, that is, in the extent to which they are differentially available to those high on the SES scale.

Money and Time

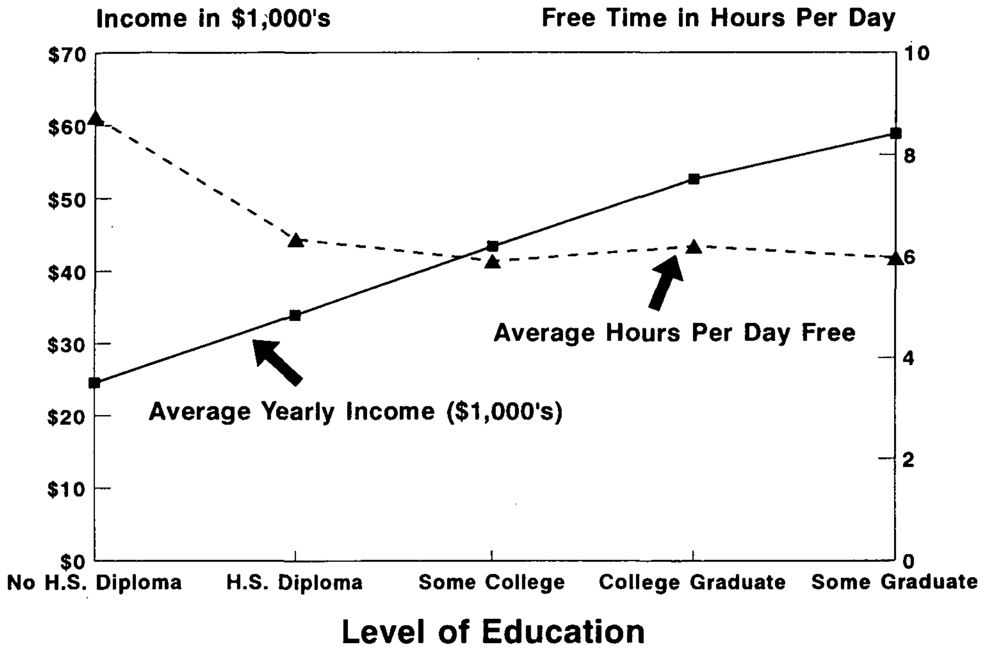
If you want to give money, you must have money; if you want to contribute time, you must have some free time. Income, of course, is one of the components of socioeconomic status. By definition, it is concentrated in the hands of the wealthy; we also know that those with more education and higher-status jobs command a disproportionate share of the wealth. We know less about the distribution of free time in relation to SES. On one hand, the rich might have more free time because they can hire others to do what most people have to do for themselves. On the other, the rich might have less free time because they accumulate their wealth by logging long hours at work. These conjectures reflect the contradictory predictions of economic theory, which holds both that an income effect would produce more leisure for the rich because they are able to purchase it and that a substitution effect would produce less because their higher wages raise the opportunity cost of free time (Mincer 1962). In fact, neither conjecture is correct: free time and SES are unrelated.

Figure 1 shows the very different relationship between money and SES (as measured by educational attainment) and time and SES. As expected, education and family income are strongly related. There is no such consistent pattern of stratification when it comes to time. Those in the least well educated group—a disproportionate number (51%) of whom are retired, keeping house, or permanently disabled—have on average more free time. Beyond this, however, greater educational advantage is associated with neither more nor less free time. If we were to consider another aspect of SES—position on the occupational hierarchy—the contrast between money and time would appear even more clearly. If working respondents are stratified into job levels based on how much formal education and on-the-job training their jobs require, we find, not surprisingly, that family income rises sharply with each step on the occupational ladder. However, this pattern is not repeated for free time: those in the least skilled jobs have almost exactly the same number of hours free time per day as do those in the highest level jobs.

What then affects the availability of free time? The answer is simple. The factors that affect free time are "life circumstances": having a job, especially a full-time one; having a spouse with a job; and having children at home, especially preschool children—all diminish the amount of free time available. Those working full-time report, on average, six fewer hours free per day; a working spouse reduces free time by about three hours; preschoolers at home reduce free time by about three hours.[23] This finding—that in contrast to money, which is of a piece with SES, the amount of free time available varies with life circumstances rather than with socioeconomic advantage—has implications for American politics. To the extent that citizen politics in America relies increasingly on modes of activity that use money rather than time as a resource, the edge enjoyed by the already-advantaged is enhanced.

FIGURE 1

Educational Stratification of Income and Free Time



Civic Skills

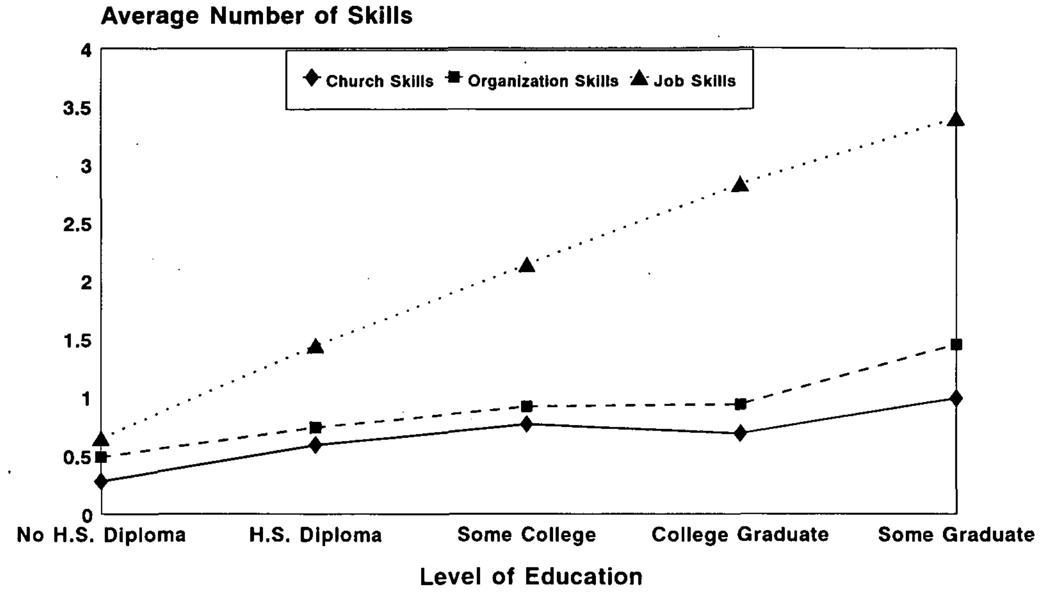
Civic skills are, in general, more likely to be possessed by the socioeconomically advantaged. Those with higher levels of education are more likely to speak English at home, to have better vocabulary skills, and to have taken part in high school government. Civic skills acquired as an adult at work, in organizations, and in church are also stratified by education. The stratification of civic skills by education, however, varies greatly across jobs, organizations, and religious institutions. Figure 2 shows the mean number of skills exercised in each of the three institutional settings for those at various levels of education. (The pattern would be the same if the measure of SES were income or occupational level.) All respondents are included in the computation of average skills whether they are involved in a particular setting or not (i.e., whether they have jobs, are affiliated with an organization, or belong to a church or not). Those who never finished high school receive few skill opportunities anywhere. Those with at least a high school diploma have more opportunities to practice civic skills on the job than in organizations or in church, presumably reflecting the fact that most people spend more time working than engaging in organizational or church activities. The net result is that in providing opportunities to exercise skills, workplaces discriminate most—and churches least—on the basis of educational attainment.

The process by which these results are achieved varies across these institutions. The opportunity to practice civic skills in an institution requires both *involvement* in the institution and a setting that provides the *chance* to practice some skills. The stratification by education for job-based civic skills comes primarily from differences in chances to practice skills and not from differences in attachments to the labor force. Those with higher education are only slightly more likely to be working than the less well educated, but among those with jobs, the better-educated are much more likely to have chances to practice skills. The stratification of skill opportunities in voluntary associations is somewhat different. The advantage of the educated in this respect comes from the fact that those with high levels of educational attainment are considerably more likely than those at lower levels to be involved with an organization. Among the involved, however, there is less difference among educational groups in the practice of civic skills. Finally, churches are most egalitarian in the civic skill opportunities they afford, and they are egalitarian in two ways: (1) there is no consistent relationship between education and church membership: those with the least education are as likely as those with the most to attend church regularly; and (2) among those who attend church, there is relatively little stratification by education in terms of who makes a speech or organizes a meeting.

The differences across the three institutions are significant for the stratification of participation in American politics. The workplace reinforces initial socioeconomic advantage as the well-educated compound their advantage by developing skills on the job. Since the educated join more organizations, voluntary associations also reinforce earlier advantage. However, organizations offer those affiliated

FIGURE 2

Average Civic Skills Acquired on Job, in a Non-Political Organization, and in Church



Among all respondents at each educational level

with them chances to practice skills with relatively little regard for educational attainment. Finally, because church attendance is not stratified by SES and because, within the church or synagogue, education plays a smaller role in who is active, religious institutions are the most egalitarian in terms of civic training. Compare, for example, the skill-developing opportunities in *workplace versus church* for a Catholic with a professional or managerial job (53% of whom graduated from college) and a Baptist with a clerical or blue-collar job (only 3% of whom graduated from college). The Catholic professional or manager practices an average of 2.89 skills on the job but only .22 skills in church. Compared to the Catholic professional or manager, the Baptist clerical or blue-collar worker averages fewer skill acts (only 1.11) on the job but much more at church (.84).

To summarize, we have considered several kinds of resources. These resources are distributed differentially across socioeconomic groups. If these resources, acquired outside of politics, affect political activity, we will have a potent explanation of the origin of disparities in participation across social groups. If the various resources are differentially useful for alternative political activities, our model will also explain why some forms of involvement are more stratified than others.

Figure 3 displays the resource model schematically. We shall not discuss it fully here but refer to it as we move through our argument. At this point, let us simply indicate that Figure 3 summarizes the way in which involvement with institutions—first in school and later on the job, in organizations, and in church—provides opportunities to acquire the resources relevant to political activity. Civic skills are a central—and, we believe, innovative—component of our model. They are also somewhat problematic. We begin our analysis of the resource model with a closer look at civic skills.

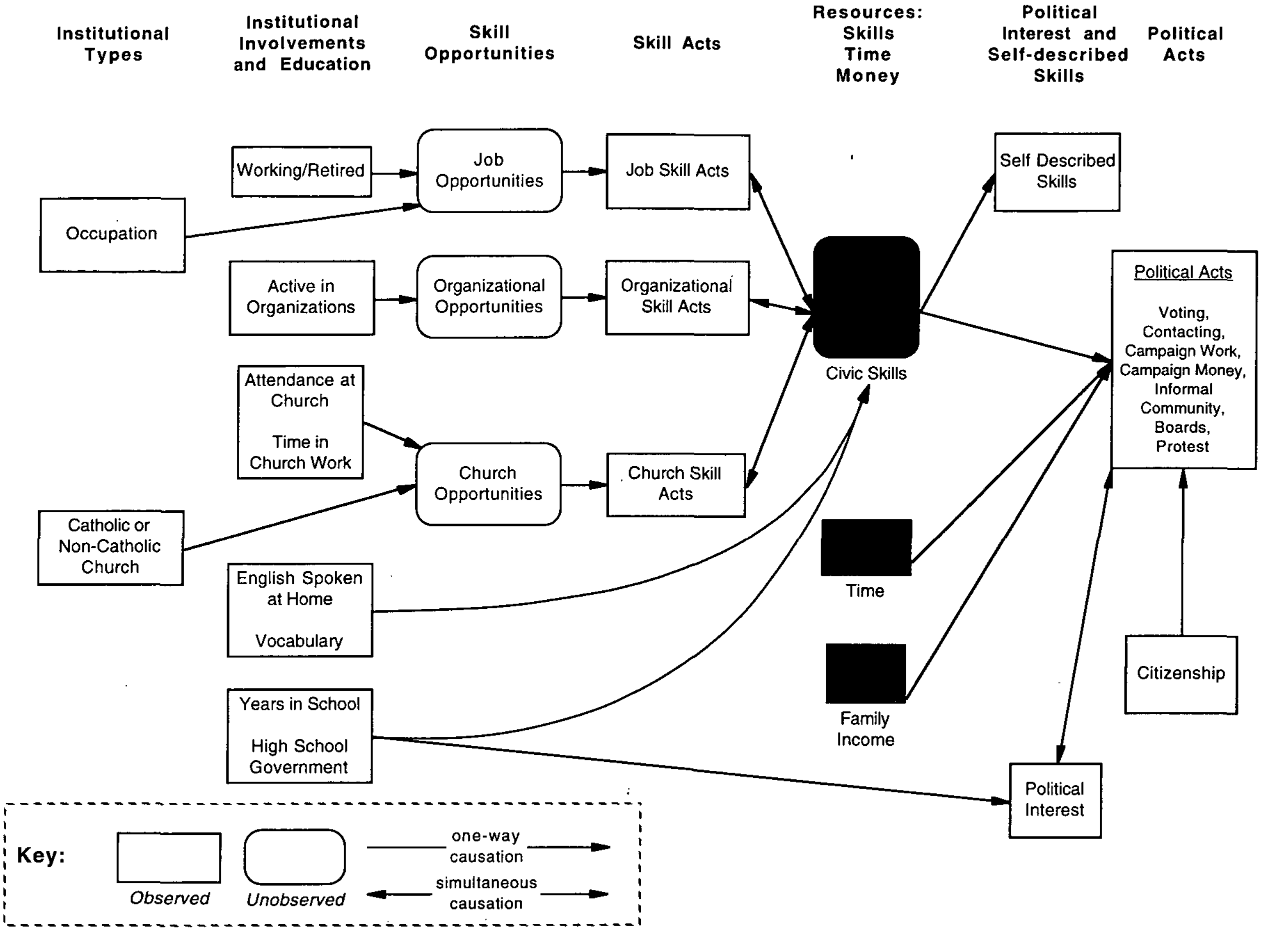
DEVELOPING CIVIC SKILLS: A LEARNING MODEL

Our measures of civic skills—educational attainment, participation in high school government, language ability, and reports on activities in adult institutions—are a disparate and somewhat indirect set. Therefore, we need to demonstrate that these are all indeed measures of civic skills and that these skills are developed or perfected in the institutions we describe. If these conditions hold, we will have a powerful set of variables that can be used to explain political activity. The task is somewhat easier for civic skills learned in the home and in school. It seems reasonable to suppose that education, language abilities, and participation in high school government constitute real measures of skills that can facilitate political participation. And we know that they are temporally prior to adult engagement in politics. However, we wish to demonstrate that civic skills are also developed in adulthood and that these skills then foster political participation.

There are three obstacles to making this case for civic skills in general and for adult civic skills in particular. First, there is a *measurement problem*. We

FIGURE 3

The Resource Model


Institutional Types
Institutional Involvements and Education
Skill Opportunities
Skill Acts
Resources: Skills Time Money
Political Interest and Self-described Skills
Political Acts
Working/Retired
Job Opportunities
Job Skill Acts
Self Described Skills
Occupation
Active in Organizations
Organizational Opportunities
Organizational Skill Acts
Civic Skills
Political Acts
Voting, Contacting, Campaign Work, Campaign Money, Informal Community, Boards, Protest
Attendance at Church
Time in Church Work
Church Opportunities
Church Skill Acts
Catholic or Non-Catholic Church
English Spoken at Home
Vocabulary
Time
Citizenship
Years in School
High School Government
Family Income
Political Interest
Key:
Observed
Unobserved
one-way causation
simultaneous causation

measure civic skills acquired before adulthood indirectly by asking about educational experiences and language abilities. It is not certain that those with high levels of educational attainment, experience in high school government, high vocabulary scores, or the ability to speak English have the capacity to communicate in political settings or to organize political activities. A similar but more severe problem applies to civic skills developed in adulthood. It seems likely that respondents who report engaging in an activity such as writing a letter or organizing a meeting have the skill to do so. However, the converse is not necessarily true; all those with the skill to write letters do not necessarily do so at work. Our measure of adult civic skills, then, really measures engaging in activities that require skills, what we shall call *skill-acts*. Our claim that skill acts measure civic skills amounts to arguing there must be at least some correlation between civic skills and the three skill-acts variables at the center of Figure 3.

Second, correlation is not enough. Even if some measure of civic skills is correlated with skill-acts, there might be a *spurious correlation problem.* A third variable (e.g., a general taste for activity) may lead individuals both to practice skills in nonpolitical institutions and to have civic skills. There may be no causal relationship between skill-acts and civic skills, and we should not draw causal arrows between skill-acts and civic skills as we do on Figure 3. Finally, there is the *locus of development problem,* the issue of where skills are developed. Even if the correlation between skill acts and civic skills is not the result of some third variable, the relationship may be due to the fact that civic skills lead to the performance of skill-acts, not vice versa. Individuals may perform skill-acts in a particular institutional setting because they brought skills with them, perhaps innate skills or skills learned elsewhere. If this is the case, civic skills will not be developed in nonpolitical institutions through skill-acts. The exercise of skill-acts in a particular institutional setting will not imply that civic skills are obtained or enhanced in that setting.

In short, to establish the proposed links between civic skills and political participation we must demonstrate that our measures indeed measure real skills; that there is no unspecified additional factor (e.g., a taste for involvement) responsible for both the performance of skill acts and political activity; and that these skills were developed or enhanced in the nonpolitical institutions where they are exercised.

One response to the measurement problem is that our measures have a good deal of face validity. This is certainly the case for education and language ability. It also applies to the exercise of skills in adult institutions. Because those who perform a skill-act are presumably learning a new skill or maintaining and improving a preexisting one by practicing it, the measure of skill-acts is very likely to be an indicator of the existence of civic skills.[24] Moreover, because we ask about skill-acts in three major secondary institutions, we capture the main opportunities people have to practice such skills. Despite these arguments in their favor, the questions about skill-acts have what might seem at first to be a disconcerting feature. Responses to them are not all highly correlated across the three domains of job, organization, and church.[25] How can these questions measure a coherent one-dimensional concept like civic skills if a person who reports performing skill-acts in one domain is not very likely to do so in another? Answering this question requires some careful modeling of how opportunities to exercise skills interact with the skills individuals already possess to produce the kinds of skill-acts our respondents report.

With some extensions, a model can also provide the basis for meeting the other two problems we have delineated. We must show that the performance of skill-acts in each domain represents an *exercise* of civic skills (not merely the reflection of some third variable) and that those who perform skill-acts thereby *develop* civic skills as well. In other words, people use preexisting civic skills (education-based organizational and communications skills as well as innate skills) to perform skill-acts. In turn, when they perform skill-acts in one institution they increase their skills so that they can engage in still more skill-acts in that or some other domain. Establishing this kind of reciprocal causation is usually very difficult in cross-sectional studies without making some arbitrary "exclusion" restrictions on which variables affect other ones but we have leverage on this problem because we have asked people about their activities in three different domains that cover most of the major opportunities adults have to gain skills. Within each domain, it is obvious that the type of institution and the level of involvement in it should affect skill-acts in that domain but not in the other domains. This provides some obvious exclusion restrictions.

Figure 3 displays what we believe to be the reciprocal causation between civic skills and skill-acts in the three domains of work, organizations, and church. We propose that an individual's preexisting *civic skills* represented by the shaded rounded box at the upper right of Figure 3 combine with *opportunities to practice skills* in each domain listed in the third column to produce the *skill-acts* listed in the fourth column. For example, such preexisting civic skills as the ability to write a letter combine with opportunities on the job to write letters to produce job skill-acts captured by our questions. In turn, these skill-acts (as shown by the arrow going from skill-acts to civic skills) develop and enhance civic skills.

As indicated by the rounded boxes on Figure 3, we do not have direct measures for several of the key variables, skill opportunities, and civic skills. Therefore, we must find proxies for them. Whether an individual gets the opportunity to practice a civic skill at work, in an organization, or at church depends upon several things. Obviously, it depends upon *the institutional involvements* listed in the second column in Figure 3—having a job or being affiliated with a voluntary association or religious institution. We measure workplace involvement by a three-point scale of employment status (not working scored as 0, part-time as 1, and full-time as 2); attachment to organizations by a three-point scale (ranging from no involvement, scored as 1, to attending meetings, scored as 3); and involvement in religious institutions by a nine-point measure of frequency of religious attendance and a six-point measure of the number of hours devoted to church activities. Beyond institutional involvements, the opportunity to practice skills also depends on the *type of institution* listed in the first column in Figure 3. Occupations, voluntary organizations, and churches differ substantially in the extent to which they afford opportunities to exercise skills germane to politics. Someone who works in a consulting firm rather than a dry-cleaning shop, who is involved in a fraternal organization rather than softball league, or who is active in a congregationally organized, rather than a hierarchically organized, church is more likely to have an opportunity to develop civic skills. For jobs we describe the type of institution by a nine-point occupation scale[26] in which higher values indicate higher-status jobs, which presumably provide more opportunities for skill-acts, and for religious institutions we use a dummy variable for belonging to a Catholic church, which provides fewer opportunities for skill-acts, presumably because of its hierarchical structure.

Obviously, it would have been preferable to have direct measures of *civic skills*, but we could hardly follow our respondents around to observe their skill in making public statements or organizing meetings. As substitute measures, we use our respondents' subjective reports of their civic skills. *Self-described skills* (the box in the upper right-hand corner of Figure 3) are measured by questions as to whether respondents believe they could write a convincing letter on a public matter, could talk well at a meeting, and would be taken seriously if they made a public statement. These three items are correlated about .32 with each other and form a reasonable eight-point scale. There are, however, potential problems with this measure. For one thing, it raises the same questions we discussed earlier with respect to measures of political efficacy (which, in fact, it resembles); that is, high self-assessments of civic skills could be the result as well as the cause of political activity. This limits the usefulness of the measure for explaining political participation but does not affect its usefulness for understanding the relationship among civic skills, skill-acts, and opportunities to exercise skills.[27] A second potential defect is of greater concern: people's self-assessments are unlikely to be fully accurate as measures of their real talents. At worst, however, the measurement error inherent in respondents' subjective reports of their civic skills would probably depress our estimates of the relationships between civic skills and skill-acts. Therefore, in this context, self-described skills are a reasonable measure of civic skills.

Estimates of the learning model for civic skills are in Appendix B. We show that there is a single dimension of "civic skills" underlying the various measures of civic skills—the three measures of adult skill-acts, the two educational experiences, and language ability. Moreover, people develop civic skills through their involvement in the institutions of adult life. Engaging in skill-acts (planning meetings, making speeches, etc.) develops civic skills that are potentially transferable to politics. We also show that the nature of the institution itself affects the number of skills exercised there—even after a number of individual characteristics that might affect the performance of skill-acts have been taken into account. This supports the notion that we are observing a real process of skill development within institutions, not merely the consequences of the attributes that people bring with them. The analysis in Appendix B demonstrates in a concrete way how churches, jobs, and organizations can serve as potential training grounds for political activity and how nonpolitical choices about jobs, organizations, and church attendance may affect political life. To demonstrate that they do, in fact, have such an effect is our next, and most fundamental, task.

POLITICAL PARTICIPATION AND RESOURCES

We have shown that accidents of birth combine with choices about jobs, family, and organizational and religious involvement to determine the resources of time, money, and skills that individuals bring to politics. We now inquire about the links between these resources and political activity. We construe political participation quite broadly and include the following in the scale:

voting in the 1988 presidential election (70% of the population)
contacting at federal or local level in last year (34%)
giving campaign money between January 1988 and the interview in Spring 1990 (24%)
working informally with others on community problems in the last year (17%)
campaign work between January 1988 and the interview in Spring 1990 (9%)
protesting during the last two years (5%)
board membership or regular meeting attender during last two years (5%)

These participatory acts vary along many dimensions. Some require the investment of time, some money. Some require skill, others do not. Some are ongoing, others episodic. And some are considered mainstream, others less so. Our survey has questions about several other activities such as voting in local elections, participating in political organizations, and phoning in to radio talk shows, but the activities listed above cover the major dimensions of political activity. Our survey also includes detailed information on the time and money devoted to political acts. Later we will use this additional information, but for the moment, we score each of these seven activities as 1 for people who engage in it and 0 otherwise. Then a simple sum[28] of the number of acts yields an average of 1.63 activities in the sample (with a standard deviation of 1.35). This suggests that the average person votes and performs part of another act. In fact, the frequency distribution of acts is skewed with 21% performing no acts, 33% one, 21% two, 14% three, 7% four, 3% five, and 1% or less six or seven acts.[29] Thus, three-quarters of the population performs between zero and two acts, and the remaining quarter is concentrated at three or four acts.

Estimating the Model

We presented the complete resource model in Figure 3, in which such resources as free time, family income, and civic skills, along with citizenship status and political interest, explain the level of political participation. We do not observe civic skills directly but have shown that they can be represented by skill-acts, language abilities, and formal educational experiences. This suggests that we can use ordinary least squares (OLS) to regress political acts on free time, family income, skill-acts, language abilities, and formal educational experiences. Proceeding in this way, however, depends upon our faith in the exogeneity and reliability of the resource measures and our willingness to make assumptions about what affects political participation. We have argued that one of the advantages of a resource explanation for political participation is that resources result from decisions about life circumstances, jobs, joining organizations, and attending church that are temporally prior to political participation. This seems obviously true for family income and free time, and it seems very likely true for adult civic skills developed at work, in church, and in nonpolitical organizations. If so, resources can be considered exogenous and OLS can be used to estimate the resource model.

This leaves us with one possible problem. If some omitted variable (e.g., a taste for participation) is correlated with both the accumulation of resources (most likely civic skills as measured by skill-acts) and political participation, then ordinary least squares will yield biased estimates. There are good reasons to believe that this might not be much of a problem in our model. First, the inclusion of participation in high school governance probably helps control for preexisting tastes for political activity. Second, we do not believe that the process that leads to involvement in a church, for example, has much to do with a taste for political participation. Yet we do believe that psychological engagement with politics matters for political participation, so that it seems sensible to add an additional control for *tastes* in the participation equation by including the sum of interest in local and national political affairs.[30] This has the virtue of allowing us to compare the relative importance of resources and psychological engagement. It has the defect, however, that it may introduce another problem. As we have discussed, political interest may be as much an effect as a cause of political activity: it may be endogenous. This may require using two-stage least squares (2SLS) to correct for biases created by the endogeneity of political interest.

Ordinary Least Squares Estimates

Before going to 2SLS estimates, it is instructive to consider the OLS estimates in Table 2, which reports regression coefficients, standard errors, and beta weights[31] for the impact of various resources in a linear model.[32] The only difference between the left- and right-hand set of columns is the inclusion of political interest in the latter equation. The results clearly demonstrate the importance of resources. Except for speaking English at home, the measures of civic skills acquired early in life (education, participation in high school government,[33] and vocabulary ability) are positively related to political activity. When citizenship is left out of the equation, speaking English at home appears to have an impact, but our analyses suggest that this is merely because it spuriously picks up the impact of citizenship with which it is correlated at .48. Citizenship must be included in the equation because it is a prerequisite for voting and might affect other kinds of participation as well. Civic skills acquired as an adult in nonpolitical institutions are also significant, making clear the role of the social institutions of civil society in creating a competent and active citizenry. Family income also matters a great deal. The only resource measure that does not have a significant effect is free time. As we shall see, when we purge free time of error, it too emerges as a significant factor in explaining political activity.

Now consider the right-hand columns reporting the equation that includes political interest. Note that with the exception of retirement, neither the degree of institutional involvement nor the type of institution is more than modestly important in determining participation. Time spent in educational, charitable, or social activities associated with a church has a small impact that is barely statistically significant but none of the rest of the institutional involvements has a statistically significant impact on political participation. Simply being involved with nonpolitical institu-

TABLE 2

Determinants of Overall Political Participation: Ordinary Least Squares Estimation

	OVERALL POLITICAL PARTICIPATION MEASURE			
	MODEL W/O INTEREST		MODEL WITH INTEREST	
INDEPENDENT VARIABLE	COEFF. (SE)	BETA WT.	COEFF. (SE)	BETA WT.
Political Interest	—	—	.261** .015	.304
Adult skill-acts				
Job	.087** (.022)	.101	.057** .021	.066
Organizational	.137** (.029)	.106	.123** .027	.095
Church	.118** (.033)	.088	.096** .031	.072
Time and money				
Free time	.000 (.007)	.002	.004 .006	.013
Family income	.051** (.009)	.112	.047** .008	.104
Institutional involvements				
Working	−.045 (.038)	−.030	−.008 .036	−.006
Retired	.388** (.090)	.090	.313** .085	.073
Organizational	.070 (.036)	.043	.031 .034	.019
Attendance at church	.010 (.011)	.021	.001 .010	.002
Time in church work	.049 (.028)	.043	.053 .027*	.047
Institutional types				
Occupation	.020 (.011)	.040	.021* .010	.042
Catholic church	.061 (.055)	.020	.086 .052	.028
Formal education				
Years of education	.145** (.021)	.164	.120** .020	.136
High school governance	.178** (.025)	.130	.118** .024	.086
Language ability				
Speaking English at home	.045 (.077)	.011	.056 .073	.014
Vocabulary score	.062** (.013)	.099	.032* .012	.051
Citizenship	.889** (.158)	.109	.790** .150	.097
Constant	−1.380** (.193)	—	−2.281** .190	—
R^2	.301		.377	
Sample size	2,438		2,429	

Source: Data from Citizen Participation Survey.
Note: COEFF. refers to the regression coefficient and SE to its standard error. BETA WT. refers to the standardized regression coefficient.
$^{*}p \leq .05$.
$^{**}p \leq .01$.

tions does not foster political activity. What counts is what happens there—in particular, whether there are opportunities to learn, improve, or maintain skills. This result elaborates in an important way our understanding of the role of nonpolitical institutions in stimulating political activity.

That political interest is related to political activity is, in itself, not very illuminating. As we have pointed out, political interest is likely to be a consequence as well as a cause of political activity. Even if we consider it only as a cause of activity, however, it is hardly astonishing that those who are interested in politics are also active.[34] What is important is that its inclusion does not supplant the effects of civic skills and family income. The relationship between resources and activity remains even with this powerful predictor of political activity in the equation.

Problems of Endogeneity

There is still room for worry, however. If political interest is endogenous, then the OLS estimates may be biased. Even if political interest is not endogenous, OLS estimates may still be biased if some unmeasured variable not captured by political interest affects both the accumulation of resources and political participation.[35] One approach to this problem (Achen 1986) is to find exogenous variables that explain skill-acts but are not also proxies for the tastes that directly cause political participation. These can then be used as instrumental variables to purge the measures of civic skills of this taste factor.

Two-stage least squares is the most efficient method for combining the instrumental variables that we need for civic skills and political interest. It is also useful for correcting for error in free time and income.[36] It requires the availability of good instruments—exogenous variables that are highly correlated with the included endogenous variables. It also requires that enough instruments be excluded from each equation to produce identification. Because we have a theory in which institutional involvements and institutional types have no direct impact on citizen participation (see Appendix B), we can use these variables as instruments in the participation equation. After all, our theory says that institutional involvements and types should be omitted from the participation equation. One might, however, wonder whether institutional involvements might also proxy a taste for participation. This suggests that it would be interesting to include these measures in the equation to see if they have an impact beyond resources. Unfortunately, this may leave us with a meager and weak set of instruments.

Our solution is to proceed in two complementary ways. We use 2SLS to estimate a model in which institutional involvements are excluded and used as instruments. Then we use 2SLS to estimate a model that excludes institutional type and religious attendance to ensure identification but that includes measures of institutional involvement for jobs, organizations, and church. These two approaches and the OLS results converge on a common interpretation in which resources and psychological engagements drive political participation.

Two-Stage Least Squares Estimates

Whereas the OLS model incorporates the possibility of direct effects from institutional involvements and types to political participation, our first 2SLS model assumes that these paths are zero. We assume this because our resource model suggests that these paths should be zero. This provides a substantial set of variables that were in the OLS equations but are excluded from the 2SLS model and used as instrumental variables: *working, retired, occupation, organizational involvement, attendance at church, time devoted to church work,* and *Catholic.* In addition, we include in the equation (and treat as exogenous) *family income, participation in high school governance, speaking English at home, formal educational experience, vocabulary score,* and *citizenship status.* This means that the three skill-acts measures and free time are treated as endogenous. Finally, we also use a set of individual characteristics that are clearly exogenous as instruments: *race* (African-American and all others), *ethnicity* (Latino and all others), *parents' education average* on a nine-point scale, *gender, number of children at home,* whether any children are of *preschool age,* and whether the *spouse works* full-time, part-time, or not at all.[37] For the equation with political interest in it, we use one additional instrument: the respondent's *political interest* as reported on a screener interview completed 6 to 12 months before the final interview.[38]

Table 3 presents data from a 2SLS analysis in which institutional involvements and types (as well as the set of demographic attributes and life circumstances listed) are used as instruments but excluded from the equation. This approach should avoid the problem of possible bias in the OLS estimates. The result for the resource model are very similar to what we found in Table 3 except that free time, now that it is purged of error, is also significant. Somewhat surprisingly, political interest and adult civic skills matter even more in the 2SLS equation than in the OLS version. We believe this is because political interest and adult civic skills are measured with error and 2SLS corrects not only for endogeneity but also for unreliability.[39] In this case, it appears that unreliability has depressed the OLS estimates.

The similarity of the coefficients for the three kinds of adult civic skills is especially striking. If the skill-act measures really measure skills and if the resources model is correct, then we would expect that skills exercised at work, in nonpolitical organizations, and in church would be transferable to politics at about the same rate. If either of these hypotheses fail, it would seem unlikely that the coefficients would be equal to one another. A test for equality of the coefficients strongly supports the conclusion that the three coefficients can be treated as equal to one another.[40] We impose this restriction on the skill-acts in the third column, and this shows that simply

TABLE 3

Determinants of Overall Political Participation: Two-Stage Least Squares Estimation

	OVERALL POLITICAL PARTICIPATION MEASURE			
	WITH ADULT CIVIC SKILLS SEPARATE		WITH SUM OF ADULT CIVIC SKILLS	
INDEPENDENT VARIABLES	COEFF. (SE)	BETA WT.	COEFF. (SE)	BETA WT.
Political interest	.420 (.030)	.489	.399** (.031)	.465
Adult skill-acts				
Job	.163** (.047)	.189	—	—
Organizational	.091** (.040)	.070	—	—
Church	.177** (.036)	.131	—	—
Sum of adult civic skills	—	—	.154** (.021)	.303
Time and money				
Free time	.044** (.012)	.150	.042** (.008)	.142
Family income	.037** (.009)	.082	.032** (.008)	.072
Formal educational experiences				
Years of education	.089** (.023)	.101	.079** (.020)	.090
High school governance	.073** (.026)	.053	.070** (.025)	.051
Language ability				
Speaking English at home	.034 (.076)	.009	.039 (.074)	.010
Vocabulary score	.010 (.013)	.016	.011 (.013)	.017
U.S. citizenship	.699** (.156)	.085	.686** (.153)	.084
Constant	−2.862** (.202)	—	−2.721** (.200)	—
R^2	.337		.345	
Sample size	2,427		2,430	

Source: Data from Citizen Participation Survey.
Note: COEFF. refers to the regression coefficient and SE to its standard error. BETA WT. refers to the standardized regression coefficient. Instrumental variables for 2SLS estimation are working at job, retired or not, occupational type, degree of organizational involvement, attendance at church, time in church activities, Catholic, years of education, involvement in high school governance, speaking English at home, vocabulary score, family income, black, Hispanic, education of parents, number of kids, preschool kids, sex, spouse work full-time, spouse work part-time, citizen, and interest in politics from the screener. The endogenous variables are therefore political interest, job skill-acts, organizational skill-acts, church skill-acts (or the sum of these three), and free time.
$^{*}p \leq .05$.
$^{**}p \leq .01$.

taking the sum of skill-acts yields a highly significant coefficient with a beta weight (.303) roughly comparable to the impact of political interest (.465).

Although we do not report the details here, we have also estimated this model with institutional involvements (working, retired, organizational involvement, attendance at church, and time in church activity) included as independent exogenous variables meant to represent tastes for participation. The sum of skill-acts is still highly significant, with a t-statistic of 3.46 and a beta weight of .394. Moreover, the coefficients for the institutional involvements in the equation are, with the exception of the effects of being retired and working, insignificant or of the wrong sign.[41] This suggests, once again, that it is civic skills that matter for political participation, not institutional involvements. In sum, our several modes of estimation show political resources to be potent for political activity.[42]

We can still conjure up other possible nuisances that could explain our results, but we believe that we have tried systematically to eliminate as many alternative explanations as possible with the data and techniques at hand. We show, for example, that skill-acts measure civic skills, that skills are developed in adult institutions, that civic skills predict participation even with controls for political interest and other confounding variables, and that a theoretically generated set of instrumental variables produce the same result. In addition, we have tried systematically, with zeal and thoroughness, to make our results evaporate by estimating many other models. For example, we have included age, length of residence, intensity of party identification, and many other variables in other specifications not reported here, and tried many nonlinear specifications. The results we report are typical of what we get; and in no case have we been able to eliminate—or even reduce much—the strong and significant results of civic skills and money.[43]

POLITICAL RESOURCES AND SPECIFIC POLITICAL ACTS

The resource model works very well for an overall measure of political participation. However, a summary activity measure presumably masks significant differences among political acts, differences that might be related to resources. We distinguish three kinds of activity: acts requiring an investment of money (through contributions to campaigns and political causes); acts requiring an investment of time (by, e.g., working in a campaign or on a community issue, taking part in a protest, contacting an official); and voting. We would expect these to differ in their resource requirements. Making a contribution obviously demands money but should require little in the way of free time and may not require skills. The time-based acts obviously demand some free time and probably require, on average, a higher level of civic skills. As the easiest political act, voting ought not to require much in the way of resources except, perhaps, some free time to get to the polls. Political interest might be especially important for voting because there are so few concrete payoffs to voting.

Voting

We begin with what is seemingly the least demanding form of political activity, voting. To construct the dependent variable, we combine two questions—one each about how often the respondent votes in local and national elections—to yield a nine-point scale. To simplify the presentation of the effects of the independent variables, we combine the three adult civic skills measures into one measure by simply taking their sum for each person.[44] The left-hand columns of Table 4 presents the results of a 2SLS analysis in which we regress the nine-point voting scale on average adult civic skills and other variables in the resource model. We use the same instruments as before. Political interest has by far the most substantial impact, with free time and citizenship also significant.[45] (The beta weights are .54, .23, and .15, respectively.) Income has no impact and civic skills have less impact (a beta weight of .21) than they do with the full participation index (where they have a beta weight of .30).

A consistent finding in the literature is the substantial impact of education on voting (Wolfinger and Rosenstone 1980; Teixeira 1992). Consequently, the statistically insignificant and incorrectly signed impact of education on turnout is especially surprising, but we believe that it is correct. Because past work has not treated political interest as a possibly unreliable and endogenous measure, we believe it has substantially underestimated the impact of interest and overestimated the direct impact of education. Indeed, we have replicated our results using National Election Studies data and validated and self-reported votes.[46] This does not mean, however, that education is unimportant. When we regress political interest on formal educational experiences and language abilities, we find that the impact of education on voting is funnelled entirely through political interest. Educational attainment (beta = .17), participation in high school government (.19), and vocabulary (.17) all have an effect on political interest. (The effect on political interest of speaking English at home (.03) is much smaller.) In summary, our work leads to a revised picture of voting as an act that is driven very strongly by political interest and that requires little in the way of money. Indeed, political interest is much more important than resources if our main project is to explain voting turnout.

Money

Our interest, of course, extends beyond voting. Making political contributions is an increasingly important mode of citizen participation (Sorauf 1988). When it comes to explaining contributions, the resource model provides striking results. We asked our respondents whether they had made contributions to electoral campaigns or, in response to direct-mail requests, to any political cause. If they had, we asked how much they gave. By adding the amounts given to campaigns and causes, we develop a measure of the amount donated to politics. The middle columns of Table 4 present the results of a 2SLS analysis in which this variable is regressed on the variables in the resource model. The results are unambiguous: the major determinant of giving money is having money. Years of education also matter, but neither free time nor civic skills[47] affect monetary contributions. Strikingly, even political interest has only a modest impact (beta weight of .08) on the amount donated to politics. In short, it is easy to explain the amount given: a contributor needs money—and little else in the way of civic skills or political interest—to give money.

Time

The impact of resources on the forms of political activity that require giving time (working in a campaign, contacting government officials, protesting, engaging in informal community activity, serving on a local governing board or attending board meetings) is quite different from the pattern we observed for voting or monetary contributions. In the right-hand column of Table 4, we present the results of an estimation of the resource model with the dependent variable as the number of time-based acts performed by the respondent. Political interest clearly matters (beta weight of .33), as does free time (beta of .09). Family income does not matter, but civic skills clearly have a significant impact. Adult civic skills exercised in nonpolitical institutions[48] (beta of .30) and participation in high school government (beta of .09) both matter. Educational attainment has only a weak impact, but this is because so much of its impact is funneled through adult civic skills and political interest. (Educational attainment is correlated at .48 with the average of adult civic skills and at .33 with political interest.)

The contrasting patterns for voting and performing time-based acts, particularly in relation to skills and interest, bear elaboration. Because formal education simultaneously stimulates political interest and inculcates civic skills, both interest and skills have significant positive bivariate relationships with two forms of participation: voting and performing time-based acts. However, these equations demonstrate that while voting appears to require interest but much less in the way of civic skills, time-based acts depend on civic skills as well as interest. In short, education affects voting not so much by imparting skills as by increasing political interest. In contrast, education and participation in high school government have an impact on the performance of time-based acts by enhancing skills.

Free time is also worth more consideration. For each of three of the time-based acts (working in a campaign, getting involved informally on a community issue or problem, and serving on a local community board or attending its meetings), we asked activists the number of hours they give to the activity

TABLE 4

Determinants of Different Types of Acts (Two-Stage Least Squares Estimations)

	VOTING (0–8)[a]		POLITICAL MONEY ($0–$5,500)[b]		ACTS TAKING TIME (0–5)[c]	
INDEPENDENT VARIABLES	COEFF. (SE)	BETA WT.	COEFF. (SE)	BETA WT.	COEFF. (SE)	BETA WT.
Political interest	.884**	.542	16.0*	.081	.191**	.326
	(.065)		(8.1)		(.023)	
Sum of adult civic skills	.200**	.209	−5.5	−.047	.103**	.298
	(.044)		(5.5)		(.016)	
Time and money						
Free time	.129**	.232	3.1	.045	.018**	.091
	(.018)		(2.2)		(.006)	
Family income	.013	.015	34.9**	.341	.009	.031
	(.018)		(2.2)		(.006)	
Educational experiences						
Years of education	−.042	−.025	12.2*	.060	.027	.045
	(.044)		(5.4)		(.015)	
High school governance	.003	.001	5.4	.017	.081**	.087
	(.054)		(6.6)		(.019)	
Language ability						
Speaking English at home	−.025	−.003	−13.3	−.015	.066	.025
	(.174)		(19.5)		(.055)	
Vocabulary score	.058*	.049	.8	.006	−.009	−.020
	(.027)		(3.3)		(.009)	
Citizenship	4.110**	.147	26.1	.014	−.002	−.000
	(.575)		(40.3)		(.115)	
Constant	−3.563**	—	−236.5**	—	−1.168	—
	(.593)		(52.5)		(.149)	
R^2	.235		.138		.202	
Sample size	2,322		2,430		2,430	

Source: Data from Citizen Participation Survey.

Note: COEFF. refers to the regression coefficient and SE to its standard error. BETA WT. refers to the standardized regression coefficient. Instrumental variables for 2SLS estimation are working at job, retired or not, occupational type, degree of organizational involvement, attendance at church, time in church activities, Catholic, years of education, involvement in high school governance, speaking English at home, vocabulary score, family income, black, Hispanic, education of parents, number of kids, preschool kids, sex, spouse work full-time, spouse work part-time, citizen, and interest in politics from the screener. The endogenous variables are therefore political interest, sum of adult civic skills, and free time.

[a] National and local.

[b] Campaign and mail.

[c] Board or meetings, informal, campaign, contact, and protest.

*$p \leq .05$.

**$p \leq .01$.

each week. When we limit the analysis to the 16% of our respondents who devote an hour or more per week to one of these activities (n = 393), we find a very strong relationship between the total number of hours given and the amount of free time available. Roughly speaking, each additional hour of free time per day leads to about one-third more hour of political activity per week.[49] Thus the amount of free time available seems especially important for the amount of time people give to activities. What we observe, then, is a two-stage process of political activation. Political interest and resources like adult civic skills have a major impact on the *decision* to participate (free time has a minor impact as well), but constraints on free time control the *amount* of the time-based political activity once this decision is made. Interestingly, the pattern for making political contributions is quite different. Income is determinative for the decision to donate, as well as the size of the contribution.

The different effects of political interest, civic skills, time, and money on participatory acts provide part of the explanation for the well-known multidimensionality of political participation (Verba and Nie 1972). Because different acts require different kinds of resources and more or less political interest, they form distinct clusters. Our model provides a way of explaining the existence of these clusters. It also demonstrates why formal education is so highly correlated with virtually every political act. Education affects political participation in at least two separate ways: for some activities, especially voting, education instills political interest and participatory motivations; for others, especially those that require time, education leads to skills that facilitate activity.[50]

THE IMPACT OF RESOURCES ON POLITICAL ACTIVITY

While it is beyond our scope here to describe the many ways that differences in resources contribute to different rates of participation across groups distinguished by income, race, ethnicity, or other characteristics, we can give a few illustrations of the effects on political activity of changes in resources.

Church Involvement. Consider an otherwise average person who has no involvement with a religious institution. If he or she joins a church and begins to attend services weekly and to devote an additional three hours a week to other church activity, this person will perform approximately two more church skill-acts (for the coefficients for church activity and attendance at church, see Table A-1), which will produce an increase of political acts from the average of 1.63 to 2.0 (see the left-hand column of Table 3). This increase represents over 25% of the standard deviation (1.35) of the summary acts measure.

Income. Consider an otherwise average person whose income goes up by $10,000. The middle columns of Table 4 suggest that in consequence, this person's political contributions will increase over 50% from an average of $66 to $101.

Free Time. Finally, consider the impact of children. If there are preschool children at home, a person loses 3½ hours of free time each day. This means, based upon the preceding analysis, that among those people who are already putting in at least an hour a week in political activities, the addition of preschool children will reduce their total time spent on informal community activity, campaign work, board membership, or attending meetings by about an hour per week.[51]

In each of these cases, the ordinary changes that people experience in their lives (joining a church and attending a Bible-study class, getting a large raise, or having a baby) affect the amount of political resources available and thus lead to significant changes in political participation. Most importantly from the perspective of understanding patterns of participation, changes in life circumstances have different impacts on time, money, and civic skills; and these resources, in turn, have different links to each kind of political act.

CONCLUSION

The model developed here demonstrates that motivations such as interest in politics are not enough to explain political participation. The resources of time, money, and skills are also powerful predictors of political participation in America. A model that includes resources has several advantages in explaining political activity. Resources can be measured more reliably than is possible with the motivations (e.g., efficacy or political interest) that often are used to explain activity. Furthermore, they are causally prior to political activity, deriving from home and school, choices about jobs and family, and involvements in nonpolitical organizations and churches. The civic skills that facilitate participation are not only acquired in childhood but cultivated throughout the life cycle in the major secondary institutions of adult life. In this way, the institutions of civil society operate, as Tocqueville noted, as the school of democracy.

The resource model permits us to go beyond the "standard SES model" in two ways. First, by moving to a more general level and specifying the resources derived from socioeconomic position that can be applied to politics, the model establishes the mechanisms that link SES to participation. In addition, by moving beyond SES and encompassing other resources not based on socioeconomic position (e.g., patterns of religious affiliation or involvement in nonpolitical organizations), we move toward an understanding of the disparities in activity among politically relevant groups distinguished by characteristics (e.g., race, ethnicity, or gender) in addition to SES.

Finally, the resource model illumines American politics. We have seen that different resources are differentially available to various politically relevant groups and differentially critical for various kinds of activity. To give a reductionist version of our findings—political interest is especially important for turnout; civic skills, for acts requiring an investment of time; and money, for acts involving an investment of money. To the extent that money is the least equally distributed resource and to the extent that making contributions has become in recent decades an increasingly important citizen activity, the character of American politics is profoundly altered.

APPENDIX A: THE QUESTIONS

SES and Social Characteristics

Income. Which of the income groups listed on this card includes the *total* 1989 income before taxes of *all members of your family living in your home*? Please include all salaries, wages, pensions, dividends, interest, and all other income. (If uncertain,) What would be your best guess?

Education.

What is the highest grade of regular school that you have completed and gotten credit for? (If necessary, add,) By regular school we mean a school which can be counted toward an elementary or high school diploma or a college or university degree.
Did you get a high school diploma or pass a high school equivalency test?
What is the highest degree you have earned?
Which of the categories on this card best describes the highest educational level (mother/father) completed and got credit for?

Race and Ethnicity. Do you consider yourself Hispanic or Latino? What is your race?

Citizenship. Were you born in the United States? (If *no,*) Are you an American citizen?

Work.

Last week, were you working full-time for pay, working part-time for pay, going to school, keeping house, or something else?
(Before you retired/In the last five years,) did you ever work for as long as one full year?
What kind of work (do you/did you) normally do? That is, what (is/was) your job called? (If necessary, probe,) What (are/were) your main duties?
Here is a list of things that people sometimes have to do as part of their jobs. After I read each one, please tell me whether or not you have engaged in that activity in the last six months as part of your job. Have you

A. written a letter
B. gone to a meeting where you took part in making decisions
C. planned or chaired a meeting
D. given a presentation or speech
E. contacted a government official?

Is your (husband/wife/partner) currently working part-time for pay, going to school, keeping house, or something else?

Family Structure.

How many children do you have *living at home* with you? Please include step- and adopted children living in your household.
Is this child under age 5? How many of these children are under age 5?

Language. What language do you usually speak at home—English or something else?

Institutional Involvements and Skills

High School Government. How active were you in school government? Were you very active, somewhat active, not very active, or not at all active?

Time.

About how many hours per day do you spend on necessary work for your home and family, including cooking, cleaning, taking care of children or other relatives, shopping, house and yard chores, and so forth? About how many hours in total do you spend in an average day on such necessary activities for home and family?
About how many hours do you spend on gainful employment in an average day, including commuting and work that you take home?
About how many hours do you spend studying for a degree or enrolled in courses for a degree in an average day?
About how many hours of sleep do you average a night?

Organizational Involvements.

Here is a list of organizations. Please read through this list and when you have finished, I'll have some questions. Are you a member of ———?
Have you attended a meeting of the organization in the past twelve months?
Does this organization sometimes take stands on any public issues—either locally or nationally?
Here is a list of things that people sometimes have to do as part of their involvement with organizations. After I read each one, please tell me whether or not you have engaged in that activity in the last six months as part of your involvement with this organization. Have you

A. written a letter
B. gone to a meeting where you took part in making decisions
C. planned or chaired a meeting
D. given a presentation or speech
E. contacted a government official?

Religious Organizations.

Now on a different subject, what is your religious preference? Is it Protestant, Catholic, Jewish, some other religion, or no religion? What specific denomination is that, if any?
Now I would like to ask you some questions about your religious activity. How often do you attend religious services?
If you average across the last twelve months, about how many hours per week did you give to (church/synagogue) work—aside from attending services?
Here is a list of things that people sometimes have to do as part of their church/synagogue) activities. After I read each one, please tell me whether or not you have engaged in that activity in the last six months as part of your (church/synagogue) activities. Have you

A. written a letter
B. gone to a meeting where you took part in making decisions
C. planned or chaired a meeting
D. given a presentation or speech
E. contacted a government official?

Vocabulary. Now we would like to know something about how people go about guessing words they do not know. On this card are listed some words—you may know some of them, and you may not know quite a few of them. On each line the first word is in capital letters—like BEAST. Then there are five other words. Tell me the number of the word that comes *closest* to the meaning of the word in capital letters. For example, if the word in capital letters is BEAST, you would say "4" since "animal" comes closer to BEAST than any of the other words. If you wish, I will read the words to you. These words are difficult for almost everyone—just give me your best guess if you are not sure of the answer.

Self-described Skills.

Imagine you went to a community meeting and people were making comments and statements. Do you think you speak well enough to make an effective statement in public at such a meeting?
If you did speak up, do you think people would pay a lot of attention to what you said, some attention, very little attention, or none at all?
Suppose you wanted to write a letter to someone in the government—perhaps your member of Congress or a local city official—on some issue or problem that concerned you. Do you feel that you write well enough to write a convincing letter expressing your point or do you feel that you do not?

Political Interest and Political Activities

Interest in Politics.

Thinking about your *local* community, how interested are you in *local* community politics and local community affairs?
How interested are you in *national* politics and *national* affairs?
[Screener item] How interested are you in politics?

Voting.

In talking to people about elections, we find that they are sometimes not able to vote because they're not registered, they don't have time, or they have difficulty getting to the polls.
Think about the presidential elections since you were old enough to vote. Have you voted in all of them, in most of them, in some of them, rarely voted in them, or have you never voted in a presidential election?
Thinking back to the national election in November 1988, when the presidential candidates were Michael Dukakis, the Democrat, and George Bush, the Republican, did you happen to vote in that election?

Campaign Work. Since January 1988, the start of the last national election year, have you worked as a volunteer—that is, for no pay at all or only for a token amount—for a candidate running for national, state, or local office?

Campaign Money.

Since January 1988, did you contribute money—to an individual candidate, a party group, a political action committee, or any other organization that supported candidates?

In your best estimate, about how much money in total did you contribute since January 1988?

Contacting.

In the past twelve months, have you initiated any contacts with a *federal elected* official or someone on the staff of such an official: I mean someone in the White House or a Congressional or Senate Office?

What about a nonelected official in a *federal government agency*? Have you initiated a contact with such a person in the last twelve months?

What about an *elected* official *on the state or local level*—a governor or mayor or a member of the state legislature or a city or town council—or someone on the staff of such an elected official?

And what about a nonelected official in a *state or local government agency or board*? Have you initiated a contact with such a person in the last twelve months?

Protesting. In the past two years, since (current month 1988), have you taken part in a protest, march, or demonstration on some national or local issue (other than a strike against your employer)?

Board Membership. Now some questions about your role in your community. In the past *two years,* since (current month 1988), have you served in a voluntary capacity—that is, for no pay at all or for only a token amount—on any official issues such as a town council, a school board, a zoning board, a planning board, or the like?

Attend Meetings. Have you attended a meeting of such an official local government board or council in the past *twelve months*?

Informal Community Work. Aside from membership on a board or council or attendance at meetings, I'd like to ask also about informal activity in your community or neighborhood. In the past twelve months, have you gotten together *informally* with or worked with others in your community or neighborhood to try to deal with some community issue or problem? (If you have mentioned this activity elsewhere, perhaps in connection with your church or synagogue, or an organization or local campaign, don't repeat it here.)

APPENDIX B: A LEARNING MODEL FOR CIVIC SKILLS

Production of Skill-Acts

People engage in skill-acts when they are presented with opportunities on the job, in organizations, or in church to write a letter, make a speech, organize a meeting, or participate in a meeting and when they have enough preexisting skills to respond positively to the opportunity. If they lack either preexisting skills or opportunities to exercise them, then they cannot carry out the activity. For each domain j, skill-acts are the joint result of skill opportunities and preexisting skills:

$$\text{Skill-Acts } j = a_j + b_j \text{ (Skill-Opportunities } j\text{)} + c_j \text{ (Skills)} + \text{error}, \quad \textbf{(A-1)}$$

where a_j is a constant and b_j and c_j indicate how skill-opportunities and preexisting skills are converted into skill-acts. This equation shows why all three measures of skill-acts can be useful indicators of skills even though they may not be highly correlated with one another. To the extent that individual *opportunities* to exercise skills in jobs, organizations, and churches are not highly correlated with one another, skill acts will not be highly correlated even though they are partly determined by individual skills. And there is no reason to expect a high correlation among institutionally based opportunities to engage in skill acts. Though skills are transferable and may be carried from one institutional setting to another, there is little reason to expect that an individual involved in one type of institution (e.g., having a job) would be more likely to be involved in a different one (e.g., to attend church).

If skills are converted into skill-acts at the same rate across all three institutional domains, then all c_1, c_2, and c_3 will be equal. This might be true, but it will only show up in our data if we have exactly the correct functional form for equation A-1. This seems unlikely.[52] It is asking too much for the c_j to equal one another, but it is not asking too much to have the c_j differ significantly from zero. If a c_j is zero, then skills would not be a cause of skill-acts in that domain even though, as we shall see shortly, skills might still be a consequence of skill-acts. It seems unlikely to us, however, that skill-acts would produce civic skills without also being a product of civic skills. Therefore, the first requirement for our model is that all c_j differ from zero.

Developing Skills

A central assertion of the resource model is that skills result from a "learning process." Not only do people engage in skill-acts because they have civic skills, they also develop skills because they perform skill-acts. When they write letters or organize meetings, people become more adept at these activities: their civic skills increase. Language ability (vocabulary score and speaking English at home) and formal educational experiences (educational attainment and participation in student government) also provide skills:

$$\begin{aligned}\text{Skills} &= e + d_1 \text{ (Skill-Acts 1)} + d_2 \text{ (Skill-Acts 2)}\\ &+ d_3 \text{ (Skill-Acts 3)} + d_4 \text{ (Language Ability)}\\ &+ d_5 \text{ (Formal Educational Experiences)} + \text{error}, \quad \textbf{(A-2)}\end{aligned}$$

where e is a constant and d_1 through d_5 indicate how skill-acts, language ability, and educational experiences are converted into civic skills. This equation will almost certainly be misspecified if we omit major institutional settings that create skills because we would expect a correlation between skill-acts in that domain and skill-acts in another domain. Thus it is of great importance that workplaces, organizations, and, churches encompass the major institutional settings where civic skills can be learned in adulthood.

If all skill-acts were turned into skills at the same rate, we would expect d_1, d_2, and d_3 to be equal. This seems unlikely, however, for several reasons: the functional form is uncertain, our measures of skill-acts miss the frequency with which people practice skills in each domain, and we did not ask in detail about the skill-acts performed. For example, we might expect those who practice skill-acts on the job to be more likely to learn from them because they spend so much time at their jobs. However, if the skill-acts performed on the job are merely routine, they would be less productive of skills. In any case, because we believe that engaging in skill-acts develops skills, we expect d_1, d_2, and d_3 to be positive but not necessarily equal.

Skill Opportunities from Institutional Involvements

Opportunities for individuals to perform skill-acts in an institution depend upon institutional involvements and institutional types:

$$\text{Skill Opportunities } j = f_j \text{ (Involvement } j\text{)} + g_j \text{ (Type } j\text{)} + \text{error}, \quad \textbf{(A3)}$$

where the intercept is zero to determine the mean of the unobserved skill opportunities measure.

Defining the Variables

To estimate the equations for the development of civic skills requires measures of skill-acts, skill opportunities in adult institu-

TABLE A-1

Determinants of Each Adult Civic Skill: Two-Stage Least Squares Estimations

	ADULT CIVIC SKILLS					
	JOB SKILLS-ACTS		ORGANIZATION SKILL-ACTS		CHURCH SKILL-ACTS	
INDEPENDENT VARIABLES	COEFF. (SE)	BETA WT.	COEFF. (SE)	BETA WT.	COEFF. (SE)	BETA WT.
Self-described skills	.595** (.045)	.561	.286** (.034)	.337	.135** (.020)	.200
Involvements in institutions						
Working at job	.742** (.034)	.434	—	—	—	—
Retired from job	−.178* (.095)	—	—	—	—	—
Organizational involvement	—	—	.731** (.032)	.458	—	—
Attendance at church	—	—	—	—	.063** (.007)	.167
Time in church work	—	—	—	—	.479** (.015)	.564
Institutional types						
Occupational type	.110** (.012)	.187	—	—	—	—
Hierarchical church	—	—	—	—	−.235** (.034)	−.104
Constant	−3.746** (.255)	—	−2.630 (.189)	—	−.987** (.133)	
R^2	.433		.301		.484	
Sample size	2,448		2,445		2,448	

Source: Data from Citizen Participation Survey.

Note: COEFF. refers to the regression coefficient and SE to its standard error. BETA WT. refers to the standardized regression coefficient. Instrumental variables for 2SLS estimation are working at job, retired or not, occupational type, degree of organizational involvement, attendance at church, time in church activities, Catholic, years of education, involvement in high school governance, speaking English at home, vocabulary score, family income, black, Hispanic, education of parents, number of kids, preschool kids, sex, spouse work full-time, and spouse work part-time. The endogenous variable in all three equations is self-described skills.

*$p \leq .05$.

**$p \leq .01$.

tions, language ability, formal educational experiences, as well as a measure of civic skills themselves. The performance of skill-acts in each of three domains is measured by a five-point scale (0–4) as described earlier.[53] We did not measure skill opportunities directly, but use as proxies institutional involvements and types described, along with all the other variables in our model, in the main text.

Estimating the Equations

Equations A-1 and A-2 are basic to a model of skill development in which skill-acts are the result of having skills and getting the opportunities to exercise them; and skills, in turn, are the result of engaging in skill-acts, language ability, and formal educational experiences. They form a system of equations in which self-described civic skills and skill-acts appear on both sides of the equations; these measures, then, are endogenous. A standard way to estimate equations of this sort is 2SLS (Hanushek and Jackson 1977). This requires finding exogenous variables excluded from each equation that can be used as instruments. These are easy to find in this system of equations. For the three skill-acts equations formed by substituting equation A-3 into A-1, we can use the measures of institutional involvements and institutional types that are not in the current equation and the measures of language ability and formal educational experiences. For example, for the equation for skill-acts on the job, we can use the measure of organizational affiliations, attendance at church, time devoted to church activities, the dummy for a Catholic church, and the measures of language skills and formal educational experiences. For the single skills equation A-2, we can use all the measures of institutional involvements and types as instruments. In addition to the instrumental variables that arise naturally from the system of equations, we also use a set of individual characteristics that are clearly exogenous. These are race (African American and all others), ethnicity (Latino and all others), family income in thousands of dollars, the average of parents' education on a nine-point scale, gender, number of children at home, whether any children are of preschool age, and whether or not the spouse is working full or part time.

Table A-1 reports the results of 2SLS estimations of the three skill-acts equations formed by substituting equation A-3 into A-1. The first row shows the impact of self-described skills on each kind of skill-act. All three regression coefficients are highly significant, ranging from .13 to .59. A change of 1.5 in self-described skills (about one standard deviation) leads to .9 more job skill-acts, .4 more organizational skill-acts, and .2 more church skill-acts. These are significant effects for self-described civic skills measured on five-point scales, and they amount to .56, .34, and .20 standard deviation changes ("beta weights") in job skill-acts, organizational skill-acts, and church skill-acts respectively.

Not surprisingly, the degree of involvement in an institution (e.g., working full-time, rather than part-time) has a lot to do with engaging in skill acts. In addition, it is striking how much the type of institution—working in a higher-status occupation or attending

TABLE A-2

Two-Stage Least Squares Estimates of Determinants of Self-Described Skills

INDEPENDENT VARIABLES	RESPONDENT'S SELF-DESCRIBED CIVIC SKILLS					
	FIRST EQUATION		SECOND EQUATION		THIRD EQUATION	
	COEFF. (SE)	BETA WT.	COEFF. (SE)	BETA WT.	COEFF. (SE)	BETA WT.
Adult skill-acts						
Job	.216** (.030)	.229	.236** (.029)	.251	—	—
Organizational	.148** (.046)	.126				
Church	.043 (.041)	.029	.093* (.038)	.063	—	—
Sum	—	—	—	—	.146** (.018)	.262
Formal educational experiences						
Years of education	.080** (.025)	.083	.102** (.024)	.105	.108** (.024)	.111
High school governance	.172** (.029)	.114	.189** (.028)	.126	.164** (.029)	.109
Language ability						
Speaking English at home	.119 (.078)	.027	.120 (.078)	.028	.129 (.078)	.030
Vocabulary score	.149** (.014)	.217	.152** (.014)	.222	.150** (.014)	.218
Constant	4.065** (.171)	—	4.008** (.171)	—	4.006** (.170)	—
R^2	.234		.231		.218	
Sample size	2,448		2,448		2,448	

Source: Data from Citizen Participation Survey.
Note: COEFF. refers to the regression coefficient and SE to its standard error. BETA WT. refers to the standardized regression coefficient. Instrumental variables for 2SLS estimation are working at job, retired or not, occupational type, degree of organizational involvement, attendance at church, time in church activities, Catholic, years of education, involvement in high school governance, speaking English at home, vocabulary score, family income, black, Hispanic, education of parents, number of kids, preschool kids, sex, spouse work full-time, and spouse work part-time. The endogenous variables are the adult civic skills (job skill-acts, organizational skill-acts, church skill-acts, and sum of skill-acts).
$^*p \leq .05$.
$^{**}p \leq .01$.

a Catholic church—matters for job skill-acts and church skill-acts respectively. In fact, moving from a Catholic to a non-Catholic church has about the same impact on church skill-acts as a one-standard-deviation increase in self-described skills, and moving from the lowest to the highest rung on the nine-point occupational ladder has a greater impact than a one-standard-deviation increase in self-described skills.

The results in Table A-1 show that self-described civic skills and skill opportunities lead to skill-acts. This means that we can use skill-acts as a rough measure of civic skills. It does not, however, prove that people actually learn such skills through their involvement in adult institutions.

Using the same instruments as in Table A-1, Table A-2 establishes this important point by showing that the three measures of skill-acts help explain self-described civic skills. Consider the results for the first equation. The coefficients for job skill-acts and organizational skill-acts are highly significant. The coefficient for church skill-acts is disappointing, but an examination of the correlation matrix for the coefficients reveals that there is a high correlation between the estimate for the impact of organizational skill-acts and church skill-acts. The correlation of −.38 between these two coefficients means that the estimation procedure found it hard to distinguish one from the other. This suggests that if we dropped one of them, the other one will "take up the slack." In the second equation, we drop the organizational skill-acts variable, and the impact of church skill-acts becomes much larger and statistically significant. When we omit both the organizational and church skill-act measures (not reported), the regression sum of squares becomes significantly smaller than when we include both in the first equation. An *F*-test rejects the notion that we should drop both variables. Finally, when we return to the first equation and do a *t*-test to assess whether the coefficients for organizational skill-acts and church skill-acts are equal, we find that despite the apparent difference, we cannot reject the hypothesis that they are identical.[54]

Taken together, these results imply that the joint effect of organizational skill-acts and church skill-acts is not zero and that the two kinds of skill-acts have identical impacts. There is good reason, then, to believe that all three skill-act variables are important determinants of civic skills. The third equation imposes the constraint that all three skill-acts have the same impact by substituting their average for each of them individually. An *F*-test implies that this is too strong an assumption, but the R-squared and other statistics suggest that it is not a bad approximation to the truth.

Table A-1 shows how having skills leads to skill-acts, and Table A-2 shows how engaging in skill-acts leads to having more skills. Table A-2 contains an additional important finding. With the exception of the measure of speaking English at home, the other quite varied measures of civic skills (formal education, vocabulary score, and participation in high school government) all relate significantly to self-described skills. This gives us confidence that we are measuring a coherent phenomenon that can be called civic skills.

Notes

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1. People also avoid politics because "they aren't allowed to participate." This was a major reason for nonparticipation in the United States at one time and it remains important in many countries.

2. Using an impressive time series of surveys, Rosenstone and Hansen (1993) emphasize the importance of mobilization by political leaders, mobilization around issues, and mobilization by political opportunities. Their data are especially well suited for showing how the changing political environment encourages or discourages political participation. Our data are especially appropriate for describing the linkages from SES to resources to participation. A complete theory of political participation must synthesize both perspectives.

3. Our approach has strong affinities with resource mobilization theory in sociology, although we concentrate upon the resources available to individuals instead of the resources available to social movement organizations. Social movement theorists argue that organizations mobilize people by the skillful use of resources; we argue that resources are a necessary condition for people becoming involved in politics. The two theories converge in their emphasis upon the crucial role of resources for political participation and in their description of the relevant resources. McCarthy and Zald (1977), for example, consider time and money and "skills in lobbying, accounting, and fund raising" as the most basic resources (pp. 1224, 1234).

4. The SES–participation relationship is well documented; the finding appears "with monotonous regularity" (Nagel 1987, 59). For citations of relevant literature, see Bennett and Bennett 1986, 183–186; Conway 1991, 21–27; and Milbrath and Goel 1977, 92. The SES–activity connection has been elaborated in multiple ways, including (1) differentiation among political acts, usually distinguishing between voting and "more difficult" political acts, with the SES–participation relationship more potent for the latter (Verba and Nie 1972; Verba, Nie, and Kim 1978); (2) analyses of the links between SES and activity, usually focusing on such motivating attitudes as political interest and efficacy (Almond and Verba 1963; Barnes and Kaase 1979; Kaase and Marsh 1979; Verba and Nie 1972; Verba, Nie, and Kim 1978) or on the different impacts of components of SES (Rosenstone and Hansen 1993; Wolfinger and Rosenstone 1980); (3) analyses of factors that diminish the impact of SES, often focusing on the mobilizing effects of organizational affiliations or such attitudes as group consciousness (Olsen 1982; Verba and Nie 1972).

5. After noting that "remarkably little effort has been devoted to explaining why certain resources matter and others do not," Wolfinger and Rosenstone unpack SES into its constituent parts and demonstrate that it is education, rather than income or occupation, that has consequences for voting; but they can go no farther than to say, "We cannot measure all resources directly; instead, we infer them from the individual's demographic characteristics" (1980, 9). This makes it hard for them to explain exactly how education has an impact on turnout. We shall follow their lead by elaborating resources and extending the analysis to forms of participation other than voting.

6. Huckfeldt and Sprague take a similar position in their careful review of the literature on contextual effects in politics. They express skepticism about "simple versions of the self-selection argument. To what extent do people choose their location in the social structure? How many of us really choose our workplace colleagues, our coreligionists, our neighbors? To the extent that we are able to exercise control over our surroundings, do we use political criteria in exercising such choice? Or do we choose a job because it pays well? A church because our parents raised us in it? And then we take the politics that accompanies the choice" (1993, 294). There is still the possibility that some resources may be produced through political activity. Although it is hard to see how political activity would generate rather than consume free time, it can produce more income (e.g., if activists gain patronage jobs or favorable treatment for their businesses). Similarly, those who take part in politics may develop political skills "on the job," that is, learn and improve political skills through their political activity. Although some activists do generate political resources through their voluntary participation, politics is such a small part of the lives of even the most active citizens that most of their income and skills must be amassed outside it.

7. Stratification theory describes the distribution of class, status, and power, and their consequences for the operation of society (e.g., Bendix and Lipset 1966; Wright 1985). We shall show how money, time, and skills flow from class and status and how these resources are converted into political participation.

8. Milbrath and Goel note: "No matter how class is measured, studies consistently show that *higher class persons are more likely to participate in politics than lower class persons. . . .* This proposition has been confirmed in numerous countries" (1977, 92; emphasis original).

9. Contrary to the logic of the theory, many people vote and join organizations even though they cannot possibly hope to recoup their costs through their gains to self-interest. If the range of self-interested benefits is, as it must be, expanded to encompass such psychic benefits as the satisfaction of doing one's civic duty, then the theory becomes much less potent.

10. Another rational choice approach dating back at least to Downs (1957) argues that lower information and transaction costs for well-educated people means that they will be more likely to engage in politics. Not much has been done to elucidate this approach. Our concept of civic skills, however, explains why transaction and information costs might be lower among the well-educated.

11. The Chicago school takes into account constraints on time as well as money (Becker 1965, 1976), the production of commodities by the household (Michael 1973; Michael and Becker 1973), and investment in human capital (Becker 1975). In this theory, individual behavior is constrained by income, time, and household production capabilities. Household production functions vary from person to person depending upon accumulated skills, but production capabilities can be improved through the investment of time and goods in human capital. This is exactly the argument we make here.

12. The question was on total income from all sources for the family. With the exception of the lowest of the 16 categories (which was scored at two-thirds of the range) and the highest (which was scored at one-and-a-half), responses were scored at the midpoint of the range in thousands of dollars.

13. We would have been able to generate more precise data if we had asked respondents to keep time-budgets instead of asking about a typical day, but this would have been too complicated and costly in a survey designed to cover a wide range of concerns. In fact, the results based on our approximations accord very well with the results contained in the literature on time use (e.g., Hill 1985). We did not ask about "free time" directly because pretesting indicated that this concept had no clear-cut meaning to respondents, whereas the time spent working, doing household work, studying and sleeping seemed meaningful to them. We concluded that there would be much more error in an ambiguous query about free time than in the total error across all of our easily understood questions.

14. On the similarities and contrasts between time and money, see Sharp 1981 and Mincer 1962.

15. There is precedent for considering the role of civic skills in facilitating participation. Strate and his colleagues (1989) demonstrate the importance of "civic competence" for voting turnout. However, the variables included in their measure of civic competence (e.g., attentiveness to politics and level of political information) are explicitly political. Therefore, we cannot be sure that they are not also a result as well as a source of activity. Wald mentions the extent to which "congregational organizations may serve as leadership training institutes for people who lack other means of exposure to organizational skills" and cites studies that find a strong relationship between attendance at church services and electoral turnout but not between religious attendance and other forms of political activity (1992, 35). In his study of parish-connected, non-Latino Catholics, Leege (1988) finds a relationship between parish activity and political activity and discusses the potential of parish activity for developing the kinds of skills we measure here.

16. Schooling affects participation in several ways: it fosters values conducive to participation, broadens social networks, and creates income-producing occupational opportunities. We shall return to these themes. Here, we focus on the skill-creating aspect of education.

17. The eight categories and their scores were (1) grammar school or less, (2) some high school, (3) high school graduate or GED, (4) some college, (5) college graduate, (6) some graduate work, (7) master's degree, and (8) PhD. or professional degree. Not much is gained by cluttering our equations with dummy variables for these categories, so we have used this eight-point scale throughout.

18. Participation in high school governance might also measure a "taste" for participation. A close relationship between "tastes" and "skills" is predicted by George Stigler and Gary Becker (1977), who argue that as people develop skills in an area (e.g., baseball, music, politics), they will be more likely to prefer the activity because they can derive more and more pleasure from it at the same cost. Whether it measures tastes or skills, participation in high school governance belongs in our model.

19. Thorndike and Gallup describe this test as a "test of verbal intelligence. . . . [that assesses] the nature of past learnings and not the ability to make novel adaptations" (1944, 78–79). The mean of 6.20 (with standard deviation of 2.15) on our vocabulary score is close to the mean of 6.51 (with standard deviation of 2.25) reported by Alwin for the 1989 GSS, which covered a slightly different population (1991, 628, table 1).

20. The vocabulary test may not be a very good measure of verbal ability for a very small fraction of our sample. The 1.8% of the sample who sometimes or always spoke another language at home besides English or Spanish (and therefore did not have the choice of being interviewed in their own language) might have done better if they had been interviewed in their native language. Our results, however, are not affected by excluding these people so we have left them in our analysis.

21. These facts suggest that the vocabulary score measures something more than just schooling, but for our purposes the exact relationship between vocabulary score and education is not important. What is important is that the vocabulary score allows us to control for verbal ability wherever or however it has been obtained.

22. Voluntary associations vary substantially in the extent to which they are involved in politics. We isolated affiliations with nonpolitical organizations as follows. Respondents were presented a comprehensive list of 20 kinds of voluntary organizations (e.g., unions, professional associations, fraternal groups, block clubs, political issue organizations). For each category for which the respondent indicated an organizational affiliation, we asked a series of follow-up questions about that organization (or, if more than one, about the one with which the respondent was most involved). Among these questions was whether the organization takes stands on public issues. We consider any organization that does not take stands on public issues to be nonpolitical. Unless otherwise specified, we are referring in our discussion to these nonpolitical organizations.

23. There is an interaction with gender: in families where both spouses work full-time, preschoolers reduce the hours at home for a woman by 1.5 hours more than they reduce the hours at home for a man.

24. The skill-act items have several attractive features consistent with the conclusion that they measure the acquisition of transferable skills. For one thing, in each domain the four items form similar Guttman scales (with differences explainable by variations in chances to perform the skill from one domain to another) indicating that they measure something common across institutions, not something specific to a particular institution. In addition, one activity—contacting a government official—included in the list asked in each domain (though we did consider it not an opportunity to gain a civic skill but an actual political act) does not scale with the other activities, whether one uses Cronbach's alpha as a criterion, loadings or communalities in a factor analysis, or the coefficient of determination for a Guttman scale. For example, the *lowest* loading for the four items in factor analyses of each domain is .527 while the loadings for contacting are .279, .446, and .385. This too is consistent with the hypothesis that these activities are a common set.

25. The correlations are .30 between job skill-acts and organization skill-acts, .09 between job skill-acts and church skill-acts, and .29 between organization skill-acts and church skill-acts.

26. By using these scales in our regressions, we are assuming a linear relationship between our dependent variables and each one of them. This turns out to be close to the truth in every case. We have tried numerous specifications where we use dummy variables for each occupation, workplace status, level of attachment to organizations, and every other independent variable defined by a scale. The results are very close to those reported here, and using the scales simplifies the reporting of results.

27. All that matters for our purposes is that self-described skills measure civic skills. Then our demonstration that self-described skills are related to the exercise of skill-acts implies that civic skills are related to skill-acts.

28. A count of activities is a simple and straightforward measure that simplifies the initial presentation and analysis. The extensive literature on the multiple dimensions of participation (Verba and Nie 1972) suggests the importance of disaggregating this simple measure, and one of the strengths of the resource model is its ability to predict separate dimensions of participation, such as who is likely to give money, work in campaigns, or engage in some other activities. The resource model does this by considering what is common across people (i.e., resource constraints) while lumping together acts with varying issue content. As with standard turnout models that typically do not take into account the multifarious issues that impel voters to go to the polls (e.g., Wolfinger and Rosenstone 1980), we assume for the resource model that it makes sense to consign to the error term the many issues that might motivate people to participate. On average, we suppose that these issue considerations are uncorrelated with the resources that constrain or enable their activity. Because we asked our respondents whether any specific issue motivated their participation, we have, however, been able to show (in work available upon request) that the resource model works for specific issue areas, as well as a heterogeneous collection of them.

29. A number of papers have noted that respondents typically overreport forms of participation like these because of a *social desirability bias* "in which cognitive dissonance can lead to a rather consistent distortion of memory in order to reinforce continued perception of oneself as a good citizen" (Cahalan 1968; see Anderson and Silver 1986; Hill and Hurely 1984; Katosh and Traugott 1981; Silver, Anderson, and Abramson 1986; Volgy and Schwarz 1984; Weiss 1986). One explanation for our results could be that these same biases inflate reports of involvement in high school governance, church attendance, and other activities leading to a spurious

correlation with political participation. We worried about this possibility but rejected it for several reasons. First, social desirability bias has to enter in a very specific way so that some subgroups are more prone to it than others for it to produce spurious results (Brady 1986). Second, the evidence we have reviewed is consistent with the notion that social desirability bias is a general human trait that is uncorrelated with specific characteristics of the respondents. This means that it would not bias our results at all (except for the intercept of our regressions because of overall overreporting). Third, if there are different forms of social desirability (some people think that church attendance is desirable, others do not, then social desirability may lead to an underestimation of the importance of some factors because of what amounts to a classic errors in variables situation. Fourth, our questions were designed to maximize true recall and to minimize false reporting by asking details of each act so that we have tried to minimize social desirability bias to begin with. Fifth, the most crucial part of our model is the relationship between skills and participation; but if all of our skills are socially desirable, then they should correlate because of this common fact. In fact, the correlations are .30 between job skill-acts and organization skill-acts, .09 between job skill-acts and church skill-acts, and .29 between organizational skill-acts and church skill-acts. The lowest of these numbers is the upper bound on the possible amount of common social desirability bias. It is hard to believe that a correlation of .09 could account for all of our results. A detailed memorandum available from the authors elaborates upon this points.

30. The sum goes from 2 (not at all interested in either local or national affairs) to 8 (very interested in both). The Pearson correlation of .54 between the two items compares favorably with the correlation of .55 between interest in national politics and a general interest in politics question on the screener. (Interest in local politics and the screener question correlate at .50.)

31. Beta weights are not a perfect way to measure the importance of a variable (Achen 1982), but they are convenient for making comparisons across variables.

32. An alternative to the linear form would be the assumption that participation requires the interaction (or product) of political interest and resources, but the logarithm of this functional form would be a linear form in the logarithms of each variable. It might be worth testing this functional form if the measures in the equation had a known metric (e.g., quantities or prices in a typical economics problem); but almost all the measures in the equation have an unknown metric, so we cannot be sure that we should take their logarithms. Our approach to this problem is to stick with the simple linear form in the text. Another approach is to include an interaction term of, say, political interest and the sum of civic skills to see if it matters. In fact, it does, with a highly significant *t*-statistic of 5.93; but we do not believe that this really takes us beyond what we already know from the linear formulation that can be considered the first term in a Taylor series approximation of a product. Still another approach is to try out many alternative functional forms including the logarithm of participation, a Poisson regression, logarithms of some of the independent variables (e.g., family income), and dummy variable versions of scales for occupation, education, work status, and many other variables. Our results remain the same under these alternative specifications.

33. Measures of involvement in high school sports or having taken high school civics courses, by the way, had no impact on political participation or on the development of civic skills as reported in Appendix B.

34. Nevertheless, when we consider various political acts separately, we shall see that political interest is not uniform in its impact on all modes of participation.

35. This describes a "triangular" system of causation in which participation is explained by resources and some other exogenous variables and resources, in turn, are explained only by the other exogenous variables. It is well known that triangular systems can be estimated consistently by OLS only if the error terms are uncorrelated. If there is some unobserved factor such as a taste for organizational activity that affects both resources and participation, then the error terms will obviously be correlated and the resources variables in the participation equation will be correlated with the error term in the equation, which leads to specification bias (Achen 1986, chap. 2).

36. So far, we have argued that we must include political interest in the participation equation and treat skill-acts and political interest as endogenous. Family income and free time also present special problems. It seems likely that our measures of these resources contain error, and this means that OLS estimates of their coefficients will be biased in unpredictable ways. The conventional wisdom is that their coefficients will be biased toward zero and that other "proxies" will pick up some of their effect. This is probably often true, but other things can happen (see Achen 1985). We solve this problem for the family income measure by using the income question on the screener to calculate the reliability of the income measure. This is then used to construct a new measure of income that is corrected for error. The family income measure that we use in all the regressions here is the predicted value from a regression of the follow-up measure on the screener measure. This amounts to a correction for attenuation. (We used the screener value for family income in the small number of cases where only the screener variables was available.) Although a reasonable measure, family income measured in this way is clearly only an approximation of the money available to an individual to use for political or other contributions. To solve this problem for free time, we treat it as endogenous and we use family size, preschool children, work status, and other variables as instruments to purge it of error.

37. The computer code and the data for all of our runs are available from the authors upon request and will be archived at the ICPSR at the University of Michigan.

38. We model the simultaneity between political interest and political participation by assuming that current participation depends directly upon current interest (and other factors, of course), which, in turn, depends upon current participation and past interest. Past interest is measured by a question on a screener interview 6 to 12 months before the final interview. In this setup, even if both interest items are measured with error, 2SLS provides consistent estimates for the participation equation. We can supply this proof upon request.

39. Another possibility is that interest and skills are picking up the effects of variables that appear in the OLS estimation but are omitted from the 2SLS version based upon our theoretical assumptions. If our theory is right, then they should be omitted. If our theory is wrong, then they should be included, but this leaves us with no instruments for solving the endogeneity problem. If endogeneity is not a problem, of course, then the OLS estimation suggests that these variables should be omitted. We are stymied, then, only in the case when endogeneity is a problem and our theoretical assumptions are wrong.

40. The appropriate χ^2 test is described by Judge and his colleagues, and the value of 1.92 with two degrees of freedom (highly insignificant with a probability value of about .35) strongly supports the null hypothesis that the coefficients are equal to one another (1985, 614).

41. The impact of free time becomes insignificant when we include dummy variables for working and retirement in the equation because these two variables are so highly correlated with free time. This suggests the fragility of this result, but it also raises questions about how we should think about the impact of work and retirement on participation. It seems likely that work increases participation through the development of skills (job skills and working are correlated at .605) and decreases participation by reducing free time (free time and working are correlated at −.626). These seem to be the main routes by which working can have an impact so that once job skills and free time are included in an equation for participation, working (and retirement) should be excluded. When we do include working in an equation with job skills, free time, and many other variables, we find that it has a negative coefficient and that free time appears to have no impact. Yet we can think of no consequence of working, other

than its reduction of free time, which would cause its coefficient to be negative. Moreover, the estimated coefficients for working and for free time are correlated at .609 with one another, suggesting that working is acting in place of free time. Of all our results, the impact of free time is the most fragile; but there is strong evidence (discussed later that free time, at the very least, affects the number of hours given to time-intensive participation once the decision to participate is made.

42. Another test of our model would be to include measures of recruitment to politics and measures of psychological engagement other than interest. In other work (Verba, Schlozman, and Brady 1995), we have estimated both OLS and 2SLS versions of our models after adding political efficacy, partisan strength, political interest, political information, and a measure of recruitment to politics. We found no substantial change in our results.

43. In the language of econometrics (Granger 1990; Leamer 1990), we have investigated the "fragility" of our results regarding civic skills and income and found that alternative specifications lead to the same result. As noted in n. 41, our results regarding free time are more fragile than the others.

44. A χ^2 test where the null hypothesis is the equality of the three coefficients for the skills variables in the voting equation yields a value of 3.36 which is not even significant at the .10 level so that we cannot reject the null hypothesis (Judge et al. 1985, 614).

45. The impact of citizenship is, of course, to be expected. We include it, rather than running the equation for citizens only (which might seem appropriate), because it enables us to see the impact of the resources over and above this obviously potent variable. In addition, we want to compare the role of citizenship in connection with voting with its impact on other acts for which it is not a requisite.

46. This becomes apparent using the 1990–93 National Election Studies panel. An OLS regression of self-reported vote for 1992 on education and interest in the campaign (there was no general interest question in 1992) yields beta coefficients of .22 and .34 respectively. Because interest in the campaign is likely to be unreliable and endogenous, the 1990 campaign interest measure is used as an instrumental variable for it, and then the beta weights go to .08 for education and .90 for interest. (The regression coefficient on education is barely significant at the .05 level in a sample of 1,242.) The 1990 campaign interest measure is an excellent instrument for 1992 campaign interest if the only problem with interest is due to its unreliability. If campaign interest is also endogenous, then we need a still better set of instruments like those used here. Nevertheless, using lagged campaign interest at least solves the reliability problem, and it must be considered better than using simple OLS, which has typically been done in the literature. We get similar results when we use self-reports about voting in 1988 from the same data set with betas of .11 for education and .44 for campaign interest. Finally, because self-reports of voting might be strongly driven by interest whereas validated votes might not be (perhaps because of some social desirability bias), we also used validated vote in 1988. (Our data file did not have validated vote for 1992.) The results were a beta of .03 for education (with a standard error larger than the regression coefficient) and .43 for campaign interest.

47. A χ^2 test where the null hypothesis is the equality of the coefficients for the three skills variables in the political money equation yields a value of 1.53, which has a probability value of about .50 (Judge et al., 1985, 614). Moreover, a χ^2 test for all three equaling zero yields 1.59 with three degrees of freedom which has a probability value of about .60. Finally, the t-statistic for the sum of the skill acts reported in Table 5 is -1.00 with a probability value of .32. The evidence is overwhelming that civic skills have nothing to do with giving money.

48. A χ^2 test where the null hypothesis is the equality of the coefficients for the three skills variables in the equation for acts requiring time yields a value of 4.35, which has a probability value of about .15, so that we cannot reject the null hypothesis (Judge et al. 1985, 614).

49. A regression of the total number of hours spent working on a campaign, getting involved informally on a community issue, and serving on a local community board or attending its meetings for just those with nonzero time on the independent variables of resources (time, income, the sum of adult civic skills, participation in high school governance, education, vocabulary score, speaking English at home), political interest, and citizenship shows that only free time matters. Skeptics might suggest that this result might be produced by some artifact in the way we measure time. Perhaps some people consistently under- or overestimate how much time they spend on activities in politics and in other parts of their lives. In fact, if this were the case, it would create a negative correlation between free time (which is measured as 24 minus the hours devoted to paid work, household maintenance tasks and child care, school, and sleep) and political activity.

50. The impact of education is broader still, affecting the networks people are in, as well as the likelihood that they will be in high-paying jobs. We explore these connections in later publications.

51. In this case, it matters whether our average person is male or female. Since a working woman loses more free time than a man when there is a preschooler at home, the impact on the amount of time she gives to time-based political activities will be that much greater.

52. It seems unlikely for two reasons. First, it seems improbable that we have hit upon the ideal way to measure skill-acts in our first try. There are probably some difficulties comparing our measures across domains. Second, equation A-1 is basically a "production function" for skill-acts from skills and opportunities. This suggests alternative functional forms. For example, skill-acts might be a function of the product (or interaction) of skills and opportunities as in a Cobb–Douglas production function, but by simply taking logarithms we could get the linear form in the text. Knowing as little as we do about the proper functional forms, it is better to stay with the simple linear formulations.

53. Those not involved in an institution were scored at zero. However, those scored at zero are not necessarily completely without skills. Hence, our observation of skills is censored by whether or not the person was involved in a particular institution. One of the reasons for constructing the model in the text is to overcome this difficulty by including the major institutions in which someone might develop skills. In this section only, we measure skill-acts in all kinds of organizations; elsewhere we refer to nonpolitical organizations only. For the job skill-acts we imputed some missing data. Description of the method and code used to impute these data can be obtained from the authors.

54. This test involves asking whether the difference between the coefficients for organization skill-acts and church skill-acts is zero. The test requires knowing the covariance between the estimates of these two coefficients, ascertained from their correlation and the standard errors for each coefficient as reported in the table. The value for the t-test is 1.45, well short of statistical significance.

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MONEY TALKS: DETERRING QUALITY CHALLENGERS IN CONGRESSIONAL ELECTIONS

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We examine the role of incumbent fundraising in deterring strong challengers. We construct a signaling model in which incumbents can use fundraising strategically to ward off quality challengers. We show, however, that only under very limited circumstances will there be an observable relationship between fundraising and challenger quality. Therefore, previous empirical tests for deterrence have systematically underestimated the effects of fundraising in decreasing electoral competition. Our analysis also suggests that by making fundraising easily observable, Federal Election Commission regulations may encourage candidates to overinvest time and resources accumulating large war chests instead of governing.

Campaign finance has become one of the most intensely studied areas of congressional elections. A voluminous empirical literature describes the patterns of incumbent and challenger fundraising and their effects on electoral outcomes.[1] Recently, theoretical inquiries have also begun to explore issues relating to campaign finance. Baron (1989a, 1989b) and Snyder (1990) investigate the relationship between service-induced campaign contributions and electoral outcomes. Denzau and Munger (1986) and Baron (1992) examine the potential tradeoff between satisfying interest groups and gaining votes. Austen-Smith (1987), Hinich and Munger (1989), and Cameron and Enelow (1990) relate campaign contributions to a candidate's policy positions and subsequent voter behavior. Baron and Mo (1993) prove that candidates are less vulnerable to interest groups' playing one off against the other when they take divergent policy positions. And Austen-Smith (1990) treats campaign spending as a signal to voters.[2]

These studies, however, take the presence and quality of a challenger in the election as given. This assumption may merit further analysis for two reasons. First, a number of studies have documented the increased electoral success that strong (experienced) challengers enjoy relative to inexperienced challengers (Hinckley 1980a, 1980b; Jacobson 1990a; Mann and Wolfinger 1980). For example, between 1946 and 1988 experienced challengers, defined as those who had previously held elected office, beat incumbents 17.3% of the time, as compared with 4.3% for inexperienced challengers. Even after accounting for the greater resources they possess, strong challengers consistently garner higher vote shares than inexperienced challengers, and this advantage has increased over the postwar period.

Second, informal accounts claim that incumbents raise large amounts of money, or *war chests*, specifically to deter strong challengers (Drew 1983; Jackson 1988; Sorauf 1992). And several studies show that challengers do enter races more frequently when they have a higher chance of winning (Banks and Kiewiet 1989; Jacobson 1990b). Thus it is possible that patterns of fundraising and competition are driven to some extent by the strategic interaction between incumbents and potential challengers. Several empirical studies have failed to demonstrate any systematic link between incumbent spending and challenger quality, but these analyses did not explicitly incorporate a model of incumbent-challenger interactions (Bauer and Hibbing 1989; Jacobson 1990a; Krasno and Green 1988). One of our central motivations here, then, is to see whether the dynamics captured by a signaling model can shed insights on how best empirically to test the relationship between campaign finance and electoral competition.

Another motive for examining incumbent fundraising and challenger entry is to investigate the effects of the 1974 amendments to the Federal Elections Campaign Act (FECA), which, in part, order all candidates to make publicly available their campaign finances. These regulations were instituted in response to the perception (fueled in part by Watergate) that campaign contributions were buying influence in Congress. By making contributions a matter of public record, legislators attempted to curb these potential abuses. But this act also created a possible *strategic* role for money: the FECA may have made it easier for incumbents to deter quality challengers by raising large amounts of easily observable funds.[3] Thus it is possible that in trying to decrease the importance of large donors, new campaign finance laws may have inadvertently created the present scramble for campaign funds.

We examine a model of campaign fundraising in which the challenger's entry decision is endogenous. Given that an incumbent has limited resources and would rather face weak opposition, fundraising may serve as a costly signal to potential challengers. To put it another way, funds raised play a dual role. They directly help the incumbent win reelection through the purchase of advertising time, campaign staff, and the like. They can also *indirectly* affect outcomes by transmitting information to the challenger about the incumbent's type. In general, we argue that to predict the incumbents' actions and the consequent pattern of electoral competition across districts, it is necessary to analyze both the direct and indirect components of fundraising. The central question is, *How will this dynamic affect fundraising, challenger entry, and electoral outcomes?*

Briefly, our results are as follows. We find that when incumbents have limited resources with which to pursue reelection, incumbent fundraising can convey information to challengers. However, only in certain, limited cases will this fundraising actually deter quality challengers from entering the race; otherwise, challenger entry decisions are unrelated to incumbent spending. We also find that the importance of money in elections rises when overall reelection probabilities fall, when candidates cannot accumulate war chests, and when total funds raised are easily observable. In fact, making fundraising more observable can only increase the total amount of funds raised.

We present a model of fundraising as a signal to potential challengers, then provide basic results on patterns of fundraising and challenger entry. We next perform a number of comparative statics exercises, and, finally, present our conclusions and the implications of our analysis for empirical research on campaign finance. Formal statements of all propositions and their proofs appear in the Appendix.

THE MODEL

We construct a model of political campaigns incorporating the challenger's entry decision. The sequence of events is as follows: first, the incumbent ("she") decides how to divide limited resources among various reelection-oriented activities, one of which is fundraising. A strong challenger ("he") who is considering entering the race then observes only the amount of funds raised by the incumbent; both the amount of resources needed to raise this money and the level of other activities remain hidden from the challenger. The potential challenger then decides whether or not to enter; if he stays out, a weak challenger enters the race instead. Next, the election is held. The incumbent's probability of winning is determined by the quality of the challenger she faces and by the amount of resources she devoted to fundraising and other activities. Finally, the winner of the election reaps the rewards of office. We now develop the model more explicitly.

Incumbents

We assume that legislators are purely electorally motivated. However, an incumbent faces difficult decisions as to how much time and energy to commit to various activities, all of which promote her reelection chances. Time spent raising money, for instance, is time taken away from constituency service. An incumbent's central problem, then, is deciding how to best allocate scarce resources in her quest for reelection.[4]

Incumbents in our model are endowed with one unit of resources. For simplicity, we assume that there are only two possible activities for which these resources can be used: fundraising and governing. In this context, governing can be thought of as any activity that helps the incumbent get reelected other than fundraising, including constituency service and working on legislation important to her district.

Incumbents differ as to the amount of resources they must invest in order to raise a given sum of money. Each incumbent has a *type* $\theta \in \{L, H\}$: type H incumbents are more efficient at raising money, type L incumbents less so.[5] Let j index a typical district. If $F^j_\theta(r)$ indicates the amount of funds raised by a type θ incumbent with the expenditure of resources r in district j, then $F^j_H(r) > F^j_L(r)$ for all r and j. That is, type H incumbents raise more than types L for any given amount of resources invested. Also, $F'_H(r) > F'_L(r)$, so that efficient fundraisers get a bigger marginal "bang for the buck" for every unit of resources invested to attract money. Type H incumbents will be called *good fundraisers* and types L *bad* to emphasize that the Hs are able to raise funds at a lower cost in terms of resources spent.[6]

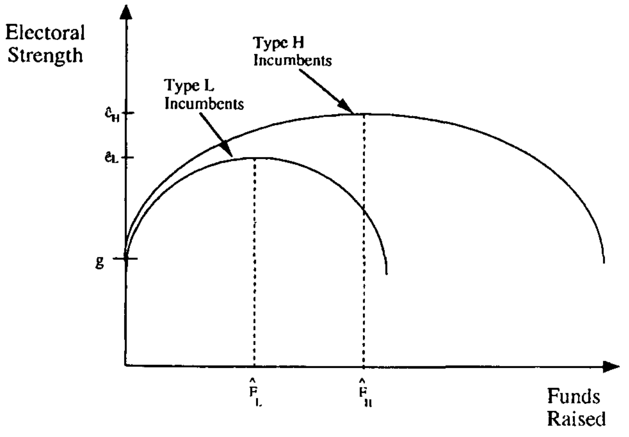
The functions $F^j_\theta(r)$ are assumed to be strictly increasing in r and the marginal returns are assumed to be strictly decreasing. That is, the first few dollars may be easy to raise, but collecting each additional dollar requires spending more and more resources. In addition, funds raised by the incumbent provide decreasing returns toward her reelection, so each additional dollar increases her probability of winning less than the previous dollar.[7]

Resources not spent fundraising are used for governing; candidates in our model have no preference for leisure. In contrast to fundraising, electoral returns to governing are assumed to be linear, at a rate g per unit of resources invested.[8] That is, an incumbent's reelection chances rise steadily as she invests more time and energy governing. Let $G^j(r) \equiv (1 - r)g^j$ denote the total amount of governing by an incumbent. We then define the *electoral strength* of a type θ incumbent who spends resources r fundraising as $e^j_\theta(r) = F^j_\theta(r) + G^j(r)$.

A candidate's electoral strength is critical to the following analysis. It captures a candidate's overall attractiveness to voters in an election, her ability to get out the vote, her policy positions, the pork-barrel projects she has brought to her district, and so on. In short, a candidate with greater electoral strength is a more formidable opponent and, all else being equal, more likely to win an election. Depending on how she allocates her resources between fundraising and governing, a candidate will increase or decrease her electoral strength. However, it is important to note that an incumbent cannot always become stronger simply by raising more money, as this takes valuable resources away from governing.[9] For the remainder of the analysis, we drop the superscripts on F^j_θ and G^j to concentrate more fully on differences between

FIGURE 1

The Relation between Funds Raised and Electoral Strength for Type L and Type H Incumbents



incumbent abilities rather than district-specific differences. We also define $e_\theta(F)$ to be the electoral strength of a type θ incumbent raising funds F.

Given their respective efficiencies of fundraising, L- and H-type incumbents will budget their time differently if they wish to achieve maximum electoral strength. Let

$$\hat{e}_\theta = \max_{r \in [0,1]} e_\theta(r) = \max_{r \in [0,1]} F_\theta(r) + G(r)$$

be the highest electoral strength a candidate of type θ can attain. Let $\hat{r}_\theta$ be the value of r that achieves this maximum, and let $\hat{F}_\theta = F_\theta(\hat{r}_\theta)$ be the corresponding amount of funds raised. Then it can be shown that if candidates of both types maximize their electoral strength, good fundraisers spend more time raising money ($\hat{r}_H > \hat{r}_L$) and, in the end, are more effective candidates than type L's ($\hat{e}_H > \hat{e}_L$).[10]

Figure 1 shows how candidates' probabilities of reelection vary with money raised and their type. If an incumbent spends no time fundraising (i.e., $r = 0$), her electoral strength is g. Note that for each candidate type the electoral strength function reaches a peak and falls off to either side. This reflects the assumption that candidates will not maximize their election chances simply by spending all their time fundraising, since at the margin they take valuable resources away from governing.[11]

Challengers

There are two types ϕ of challengers that an incumbent can face: strong ($\phi = S$) and weak ($\phi = W$). The incumbent's probability of winning an election depends on her electoral strength and the quality of her challenger: $P \equiv p(e, \phi)$. This function is left as general as possible; all that we assume is that it is increasing in e and that $p(e, S) < p(e, W)$ for any level of incumbent electoral strength e. That is, strong challengers have a greater probability of beating any given incumbent than do weak challengers. Let $P_H = p(\hat{e}_H, S)$ and $P_L = p(\hat{e}_L, S)$. Then P_H $[P_L]$ is the greatest probability of reelection that a type H[L] incumbent could have against a strong challenger.

Challengers cannot observe an incumbent's type θ directly, but they do have prior probabilities over θ. Let q be the ex ante probability that the incumbent is type H and $1 - q$ be the probability that she is type L. The value of q might be derived from the overall proportion of incumbents who are good fundraisers. Or it might incorporate knowledge of a specific incumbent's efficiency revealed in past elections; in this case, contests for open seats will differ strategically from contests with an incumbent. We assume only that changing political conditions, interest group contributions, and competition for resources make the challenger uncertain about the incumbent's type in any given election cycle.

The strong challenger observes the total amount of funds raised by the incumbent, updates his beliefs as to the incumbent's type, and then decides whether or not to enter the race. Let $\mu(F)$ be the challenger's posterior that $\theta = H$ when he observes total funds raised of F. If the challenger could observe both F and G (or F and θ), he could infer the incumbent's electoral strength. The fact that G is unobservable means that challengers may face uncertainty when making their entry decision. That is, $\mu(F)$ may be strictly between 0 and 1.

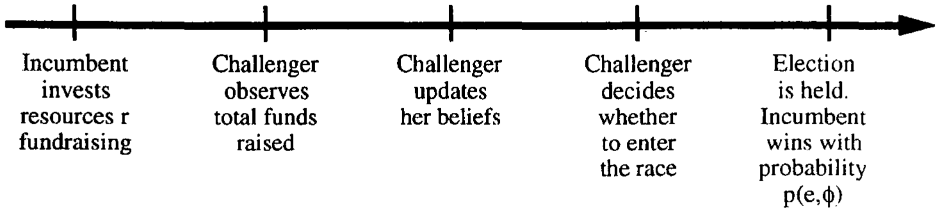
Each challenger has a reservation utility, U_S and U_W respectively, representing his options if he stays out of the race. We assume $U_S > U_W = 0$, thus making the weak challenger a default candidate: if the strong challenger refuses to run, then the weak type will enter.[12] If the value of holding office is V, then the strong challenger will enter whenever $[1 - P_S]V \geq U_S$, where $P_S = \mu(F)p(e_H(F), S) + (1 - \mu(F))p(e_L(F), W)$ is a strong challenger's expectation that the incumbent will beat him in the election after seeing fundraising F. This is just the usual expected utility calculation for challenger entry. Jacobson and Kernell (1983), for instance, use it as their basis for analyzing "strategic challengers."

There is a critical value of P_S for which the strong challenger is just indifferent to entering a race or not; this is the value: $\bar{P} = 1 - U_S/V$. Thus, strong challengers will enter a race if $P_S < \bar{P}$ (recall that P_S denotes the *incumbent's* probability of winning). $\bar{P}$ will rise when the value of office increases or reservation utilities fall; thus, $\bar{P}$ can be loosely interpreted as the strong challenger's willingness to challenge the incumbent.

Recall that the incumbent's chances of winning are related to both her electoral strength and the quality of her challenger. An incumbent seeking to maximize the probability of winning an election will not necessarily act to maximize her electoral strength. Of course, all else being equal, she would rather be a stronger candidate, but we will analyze cases in which the possibility of signaling allows the incumbent to trade off greater electoral strength for a weaker opponent.

FIGURE 2

Sequence of Events


Incumbent invests resources r fundraising
Challenger observes total funds raised
Challenger updates her beliefs
Challenger decides whether to enter the race
Election is held. Incumbent wins with probability p(e,ϕ)

Electoral Competition

Figure 2 summarizes our model and details the sequence of events. First, the incumbent of type θ chooses resources r to devote to fundraising; the remaining $1 - r$ goes toward governing. This gives her electoral strength $e_\theta(r)$. The strong challenger ($\phi = S$) observes only $F_\theta(r)$; given this information he (possibly) updates his beliefs about θ and decides whether or not to run. If he stays out, then the incumbent faces a weak challenger ($\phi = W$). Finally, an election is held, and the incumbent wins with probability $p(e_\theta(r), \phi)$.

We solve for Perfect Bayesian equilibria [PBE's] of the game played between challengers and incumbents. Let $\sigma_S : [0, F_H(1)] \to [0, 1]$ be the probability that a strong challenger enters as a function of the observed level of funds raised. Let $\sigma_\theta : [0, F_\theta(1)] \to [0, 1]$ be the probability that type θ raises each possible level of funds. Finally, recall that $\mu : [0, F_H(1)] \to [0, 1]$ is the posterior probability of the S challenger that $\theta = H$ given the level of funds he observes. A *Perfect Bayesian equilibrium* for our model is a set of strategies and beliefs $(\sigma_S, \sigma_H, \sigma_L, \mu)$ that satisfies

$$\sigma_\theta(F) > 0 \Leftrightarrow F \in \arg\max_F [\sigma_S(F) p(e_\theta(F), S) + (1 - \sigma_S(F)) p(e_\theta(F), W)], \quad \textbf{(i)}$$

$$\sigma_S(F) \in \arg\max_{\sigma_S \in [0,1]} [(1 - \sigma_S) U_S + \sigma_S V(\mu(F) p(e_H(F), S) + (1 - \mu(F)) p(e_L(F), S))], \quad \textbf{(ii)}$$

For all F such that $\sigma_H(F) + \sigma_L(F) > 0$,

$$\mu(F) = \frac{q\sigma_H(F)}{q\sigma_H(F) + (1 - q)\sigma_L(F)}. \quad \textbf{(iii)}$$

The first condition states that an incumbent will raise an amount of funds with positive probability only if that amount maximizes her reelection chances, given the strong challenger's entry strategy. Condition ii says that the strong challenger, in turn, will only enter those races where entering is at least as good as staying out, given his expectations over the incumbent's types. Finally, the last condition requires that these expectations be updated rationally from the challenger's priors according to Bayes' Rule.

Unfortunately, the requirement that the equilibrium be a Perfect Bayesian equilibrium is not, by itself, sufficient to select a unique equilibrium in all circumstances. The main problem is that condition iii does not restrict beliefs off the equilibrium path, making many patterns of behavior individually rational for all players. Even worse, as will be demonstrated in the discussion following Theorem 1, the multiplicity of equilibria prevents us from conducting meaningful comparative statics exercises. To increase the predictive power of our model, then, we employ the following equilibrium selection criterion:

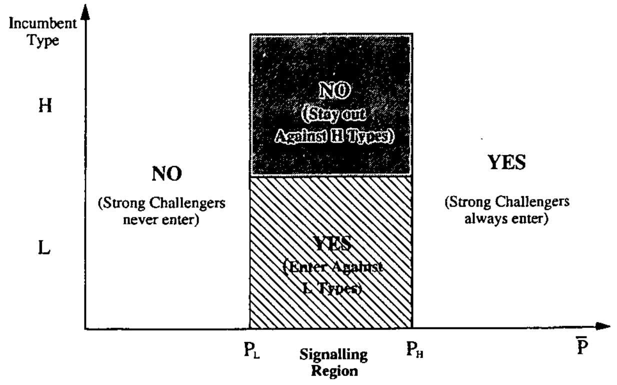
Equilibrium Refinement. *The equilibrium played is among those PBE's which maximize the reelection probability of a type H incumbent.*

The subject of selecting equilibria for signaling games has generated a considerable literature, without having reached a consensus.[13] In general, the choice of a selection criterion should be motivated by the specific features of the model being investigated. In our setting, incumbents would like to convince the challenger that they are efficient (type H) fundraisers. Therefore, an equilibrium might be thought of as unreasonable if a type H incumbent could deviate from the proposed equilibrium and make the following speech to the strong challenger: "Even though you may believe that I am a type L for taking this action, I will take it anyway. Further, only a type H incumbent would possibly gain from deviating in this manner, so you should believe that I am in fact a type H, despite your stated equilibrium beliefs." Our refinement effectively eliminates all such equilibria, since the equilibrium selected is the optimal one from a good fundraiser's point of view.[14]

Given the sequence of events and our equilibrium concept, when will a strong challenger enter the race? The answer is illustrated in Figure 3. The horizontal axis shows $\bar{P}$, and the vertical axis gives the incumbent's type. Recall that when $P = \bar{P}$, a type S challenger is just indifferent to entering the race. Under our assumptions, this cutoff point is exogenous to the model, depending only on U_S and V. Recall also that P_L is the probability that a type L incumbent beats a type S challenger if she maximizes her probability of winning. So if $\bar{P}$ is less than P_L, then even bad fundraisers can keep type S challengers out of the race by setting $F = \hat{F}_L$. Type H incumbents can also avoid strong challengers by raising $\hat{F}_H$. Thus if $\bar{P} < P_L$ and all incumbents maximize their electoral

FIGURE 3

Strong Challenger's Entry Decision as a Function of $\bar{P}$ and Incumbent Type θ


Incumbent Type
H
L
NO
(Strong Challengers never enter)
NO
(Stay out Against H Types)
YES
(Enter Against L Types)
YES
(Strong Challengers always enter)
P_L
Signalling Region
P_H
$\bar{P}$

strength, no strong challengers will enter the race. This is the meaning of the word *no* in the leftmost region of Figure 3.

Similarly, the rightmost region is labeled *yes* to indicate that when $\bar{P} > P_H$, even a good fundraiser will be challenged. Since no incumbent can influence the challenger's behavior, they might as well maximize their electoral strength and obtain the highest probability of winning the election. Theorem 1 summarizes these two cases, where the challenger's entry decision is independent of the incumbent's type.

THEOREM 1: CHALLENGER ENTRY INDEPENDENT OF INCUMBENT FUNDRAISING. *If* $\bar{P} < P_L$, *(1) type L incumbents raise* $\hat{F}_L$, *(2) type H incumbents raise* $\hat{F}_H$, *and (3) strong challengers never enter the race. If* $\bar{P} > P_H$, *(1) type L incumbents raise* $\hat{F}_L$, *(2) type H incumbents raise* $\hat{F}_H$, *and (3) strong challengers always enter the race.*

Thus the only conditions under which an incumbent's actions could possibly deter a strong challenger is when $P_L \leq \bar{P} \leq P_H$. Here, strong challengers would enter the race against a type L incumbent but not a type H incumbent. The strategic use of funds to deter challengers, if it occurs at all, must occur in this middle range; at the extremes the challenger's behavior is independent of the incumbent's fundraising. Accordingly, we label this middle range the *signaling region*, as shown in Figure 3. The possibility of incumbents' altering their fundraising in order to change challengers' behavior, therefore, only exists when the challenger would take different actions depending on the incumbent's true type. When challengers' incentives to run are very low, incumbents need not try to deter them; when they are high, any attempt to deter would be futile. Only in the middle range is there both the need for and the possibility of deterrence.

Although Theorem 1 is quite intuitive, the results use our equilibrium refinement. To see why the refinement is necessary, suppose that $p(e_L(\hat{F}_H), S) < \bar{P}$; that is, a strong challenger would enter against a type L incumbent who raises $\hat{F}_H$. Then, even if $\bar{P} < P_L$, there exist PBE's where $\sigma_H(\hat{F}_H) = 0$! In particular, define F^* by the condition that $p(e_L(F^*), S) = \bar{P}$. Then the following is a perfect Bayesian equilibrium:

$$\mu(F) = \begin{cases} 1 & F = F' \\ 0 & \text{otherwise,} \end{cases}$$

$$\sigma_S(F) = \begin{cases} 0 & F \leq F^* \quad \text{or} \quad F = F', \\ 1 & \text{otherwise,} \end{cases}$$

$\sigma_L(\hat{F}_L) = 1$ and $\sigma_H(F') = 1$, where F' is some amount such that $p(\hat{e}_H, S) < p(e_H(F'), W)$ and $p(e_H(F^*), W) < p(e_H(F'), W)$.

In this PBE, no type of incumbent plays $\hat{F}_H$. Therefore, $\mu(\hat{F}_H)$ is unrestricted by condition 3. The above PBE is supported by the belief of the challenger that when he sees $\hat{F}_H$, he is facing an L-type incumbent with certainty. These beliefs seem quite odd: a type L incumbent has no incentive to weaken herself by raising $\hat{F}_H$ since she faces a weak challenger when she raises her optimal level of funds, $\hat{F}_L$. On the other hand, a type H incumbent would benefit from raising $\hat{F}_H$ instead of F' if he could still face weak challenge. Nonetheless, the PBE given above is in fact an equilibrium, since, given the challenger's strange beliefs, both incumbent types are acting optimally.

These unintuitive equilibria make predictions difficult. One would like to say, for instance, that as a type H incumbent's efficiency of fundraising increases, she raises more money in equilibrium. However, in the example given above, F' may be either greater or less than $\hat{F}_H$. So if we allow a different equilibrium to apply after type H incumbents become more efficient, then the funds raised may either increase or decrease. After applying our refinement, however, a unique level of fundraising can be predicted ($\hat{F}_H$), and this level does in fact rise as incumbents' fundraising efficiency rises.

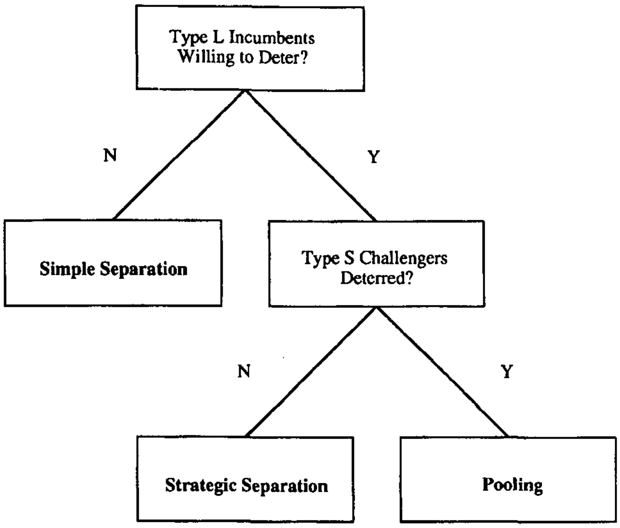
STRATEGIC FUNDRAISING

We investigate the signaling region in Figure 3. It describes incumbent and challenger behavior when strong challengers prefer to enter races against type L incumbents but not type H. The analysis will proceed by steps, as shown in Figure 4. For incumbent fundraising to deter quality challengers, two conditions must hold. First, type L incumbents must be willing to deter strong challengers. Of course, incumbents always prefer weak opposition; the question is whether or not bad fundraisers are willing to invest the necessary resources to ensure a type W challenger. If not, fundraising serves as a perfect signal of incumbent type.

If type L incumbents *are* willing to invest the necessary resources, we next ask whether strong challengers are deterred from entering a race where all incumbents raise the same amount of money regardless of their type. If so, then strong challengers

FIGURE 4

Analysis of Equilibria When Challengers Prefer To Enter Only against Type L Incumbents


Type L Incumbents Willing to Deter?
N
Y
Simple Separation
Type S Challengers Deterred?
N
Y
Strategic Separation
Pooling

are deterred from entering. If not, then in equilibrium fundraising again serves to separate incumbent types. Thus, we shall outline three possible signaling equilibria, termed simple separation, pooling, and strategic separation. We examine each case in turn.

Case 1: Fundraising as a Perfect Signal

Whenever different types of incumbents take different actions, they are said to be *separating*. So when each incumbent type θ raises $\hat{F}_\theta$ in funds, this is one kind of separating behavior. Then even though the challenger cannot directly observe the incumbent's type, observing the incumbent's fundraising is sufficient for the challenger to perfectly infer her type.[15] This motivates the analysis to follow: funds raised may serve as a *signal* to the challenger.

By copying the good fundraisers and raising $\hat{F}_H$, a type L incumbent will be spending time and resources fundraising that she would rather be spending governing. On the other hand, by expending this extra effort, she may deter a good challenger. Since the overall probability of the incumbent's winning the election depends on both her resource allocation and the quality of her opponent, the question is whether the possible deterrent effects of extra fundraising outweigh its costs in terms of forgone governing. Then a type L incumbent will wish to copy the H types if

$$p(e_L(\hat{F}_H), W) > p(\hat{e}_L, S). \tag{1}$$

Condition 1 states that a type L incumbent has a higher probability of winning if she raises $\hat{F}_H$ and gets weak opposition than if she maximizes her electoral strength and faces a strong challenger. That is, if Condition 1 holds, then type L's are willing to run at less than full *electoral* strength because the presence of a weak challenger more than makes up the difference. Whenever different types of incumbents take the same action, they are said to be *pooling*. Condition 1 will be called the *pooling condition*, since it defines the circumstances under which type L incumbents would wish to imitate, or pool with, type H incumbents.

Before we analyze behavior when the pooling condition holds, we should make explicit what happens if it does not. In this case, type L incumbents do not prefer to raise $\hat{F}_H$ to deter strong challengers. We call this case *simple separation*. So even though the incumbent's type is not directly observable, the outcome of the game is *as if* it were. This result shows that informational asymmetries do not necessarily confer an advantage on the better-informed party. Even though incumbents know more about their type, their equilibrium actions may reveal this information perfectly, in which case outcomes are exactly as they would have been with complete information.

THEOREM 2: CHALLENGER ENTRY WHEN FUNDRAISING IS A PERFECT SIGNAL. *If* $P_L < \tilde{P} < P_H$ *and* $p(e_L(\hat{F}_H), W) \le p(\hat{e}_L, S)$, *(1) type L incumbents raise* $\hat{F}_L$, *(2) type H incumbents raise* $\hat{F}_H$, *and (3) strong challengers enter the race only against L incumbents.*

The case of simple separation corresponds to Jacobson and Kernell's (1983) theory of "strategic politicians." They note that challengers will rationally enter only those races in which they have a high enough chance of winning and conclude that stronger incumbents draw weaker opposition. This is indeed the case in the simple separation equilibrium: good fundraisers raise more money and receive weaker challengers than bad fundraisers, and neither type distorts its fundraising to deter challengers. However, it should be stressed that this is only one possible outcome to our game. In the *no* and *yes* equilibria in Figure 3, incumbent type was unrelated to challenger quality. And in other cases, as shown below, type L incumbents *are* willing to deter strong challengers, thus upsetting the simple inverse relationship between incumbent and challenger strength.

Case 2: Fundraising To Deter Entry

Now assume that bad fundraisers would imitate good fundraisers if this would deter strong challengers. If all incumbents raise $\hat{F}_H$, then fundraising is no longer an informative signal.[16] Challengers who originally assigned prior probability q to an incumbent being type H will still assign the same probability after seeing the incumbent's fundraising. If the type S challenger then enters the race, his expected probability that the incumbent wins the election is

$$\tilde{P} = q \cdot [p(\hat{e}_H, S)] + [(1 - q) \cdot p(e_L(\hat{F}_H), S)].$$

The derivation of $\tilde{P}$ is straightforward: with probability q, the strong challenger will face a type H incumbent at her maximum electoral strength. On the other hand, with probability $1 - q$ he will face a weak incumbent who is made even weaker by having deviated from her optimal fundraising level ($\hat{F}_H$ instead of $\hat{F}_L$).

Since a challenger will refrain from entering whenever $E(P|F) > \bar{P}$, strong challengers will be *deterred* by a pooling strategy if

$$\tilde{P} \geq \bar{P}. \tag{2}$$

Equation 2 is the challenger's counterpart to the pooling condition; for pooling to be an equilibrium, type L incumbents must be willing to copy type H's, *and* strong challengers must be deterred by pooling. Note that as q—the probability that the incumbent is type H—rises, so does $\tilde{P}$. So there is a critical value $\tilde{q}$ for which, if all incumbents raise $\hat{F}_H$, type S challengers are just indifferent to entering. For $q > \tilde{q}$, in which case there is a high probability the incumbent is of type H, the strong challenger will stay out.

Since the type L incumbents run at less than maximum electoral strength in the pooling equilibrium, we say that they pay the *incentive costs of pooling*: they raise more than $\hat{F}_L$ in order to affect challengers' behavior. Of course, these costs are less than the benefit they receive from decreased electoral competition, so their overall probability of winning rises. These results are summarized as follows:

THEOREM 3: CHALLENGER ENTRY DETERRED BY INCUMBENT FUNDRAISING. *If* $P_L < \bar{P} < P_H$, $p(e_L(\hat{F}_H), W) > p(\hat{e}_L, S)$, *and* $q > \tilde{q}$, *(1) type L incumbents raise* $\hat{F}_H$, *(2) type H incumbents raise* $\hat{F}_H$, *and (3) strong challengers enter the race only if they observe fundraising less than* $\hat{F}_H$ *(hence none enter).*

Theorem 3 captures the possibility that incumbents may raise more money specifically to deter strong challengers. In this case, large war chests may indeed decrease the quality of the candidates in congressional elections. However, the logic of deterrence in our model is not straightforward. Instead of raising extra money to prove their strength to challengers, incumbents raise extra money in order to *hide their weakness*. As the population of good fundraisers in Congress rises, this strategy of "slipping in under the noise" becomes increasingly attractive for less gifted fundraisers.

Case 3: Fundraising as Predatory Pricing

We are left with one final case to consider. When the pooling condition holds but $q < \tilde{q}$, equilibrium behavior requires the *good* fundraisers to increase their fundraising to some level $\dot{F}_H > \hat{F}_H$. True, this lowers their effectiveness, but it discourages type L's from imitating them. Depending on parameter values, there are two possibilities for the final equilibrium. First, some—but not all—type L's may still pool with the type H's. This is a mixed strategy equilibrium in which type L incumbents randomize as to whether they raise $\hat{F}_L$ or $\dot{F}_H$. Strong challengers always enter when they see $\hat{F}_L$ (as this indicates a weak incumbent), and they enter probabilistically when they see $\dot{F}_H$. In equilibrium, type L incumbents are just indifferent between raising $\hat{F}_L$ and $\dot{F}_H$, and strong challengers are just indifferent to entering or staying out upon observing $F = \dot{F}_H$. Note that in this equilibrium, strong challengers enter against type H incumbents in some cases.

Second, type H's may raise so large an amount that the types L no longer have an incentive to copy their behavior. In fact, this is just the limiting case of the mixed strategy equilibrium: as type H's increase the value of $\dot{F}_H$, fewer type L's wish to imitate them, and fewer strong challengers enter. This type of behavior weakens the type H's since they depart from their maximum effectiveness, but the payoff from deterring strong challenges raises their overall probability of reelection.

We call this case *strategic separation*.[17] In simple separation (Theorem 2), no incumbent has incentives to raise more than $\hat{F}_\theta$, while strategic separation requires type H incumbents to raise more than $\hat{F}_H$ in funds. In this equilibrium, therefore, we say that the type H incumbents pay the *incentive costs of separation*. Type H incumbents would be better off if they could indicate their type directly to the challenger, but given that this is impossible, they must prove their efficiency in fundraising by gathering so much money that few type L incumbents would want to imitate them. These results are summarized as follows:[18]

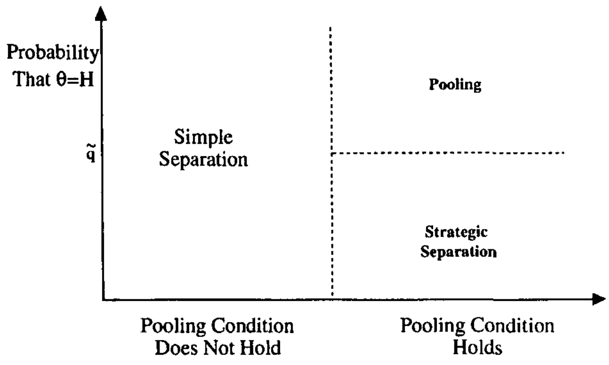
THEOREM 4: CHALLENGER ENTRY WITH STRATEGIC SEPARATION. *If* $P_L < \bar{P} < P_H$, $p(e_L(\hat{F}_H), W) > p(\hat{e}_L, S)$, *and* $q < \tilde{q}$, *(1) type H incumbents raise an amount* $\dot{F}_H > \hat{F}_H$*; (2) for some parameter values all type L incumbents raise* $\hat{F}_L$*, and for other parameter values type L incumbents randomize between* $\dot{F}_H$ *and* $\hat{F}_L$*; and (3) strong challengers enter the race if they observe* $\hat{F}_L$ *and they enter with some probability less than 1 if they observe* $\dot{F}_H$.

Strategic separation is campaign finance's equivalent to predatory pricing (Joskow and Klevorik 1979; Saloner 1981); good fundraisers take actions that bad fundraisers find too costly to imitate. This highlights a subtle point in our approach to campaign finance: competition develops not only between incumbents and potential challengers but also among the possible types of incumbents. Good fundraisers as a group may not want to be confused with bad fundraisers, who are more likely to attract strong challengers. Thus they alter their behavior clearly to distinguish themselves from the type L's. By "running away" from potential imitators, type H's thus ensure that they will receive weak challengers, but to do so they must raise the amount of resources they invest in fundraising above their preferred level, just as firms must charge prices lower than their profit-maximizing price to keep higher-cost competitors out of the market.

The signaling equilibria are illustrated in Figure 5. The horizontal dimension indicates whether or not the pooling condition holds. The vertical axis shows the prior probability q that an incumbent is of type H. One can think of the x-axis as the incumbents' view of

FIGURE 5

Different Possible Outcomes with Incomplete Information



potential challengers, in terms of the difference in reelection probabilities they represent; while the *y*-axis is the challengers' view of incumbents, in terms of the probability of meeting each type in an election.

One of the basic questions motivating this essay is whether the asymmetric information inherent in fundraising confers a strategic advantage to the incumbent. The analysis indicates that the answer is *not necessarily*. In two of the three areas in Figure 5, separating equilibria exist, in which case bad fundraisers cannot avoid a strong challenge. In fact, in the strategic separation equilibrium with mixed strategies, good fundraisers may receive strong challengers, even though these challengers would stay out of the race under complete information. Overall, then, there is no clear relation between fundraising, private information, and challenger quality, even when all actors are strategic and incumbents would like to deter quality challengers.

COMPARATIVE STATICS

We now conduct a series of comparative statics exercises on the equilibria already derived. Specifically, we investigate how changes in national electoral tides, war chests, and fundraising observability lead to changes in patterns of electoral competition and fundraising. In general, there are two types of changes that can result. First, equilibrium behavior may move from one region of Figure 3 to another as challengers become more or less willing to enter any given race. Second, behavior within the signaling region, detailed in Figure 5, may shift from one equilibrium to another.

National Forces

We first investigate the impact that changes in the electoral environment will have on patterns of candidate competition. We call factors that change incumbents' chances of being reelected "national forces," including national economic conditions, a general anti-incumbent movement to "throw the bums out," or the presence of a strong presidential candidate at the top of the ticket. Referring back to Figure 3, a rise in anti-incumbent bias decreases the probability that any given incumbent will win, shifting both vertical lines (P_L and P_H) to the left. This may in turn shift a fixed $\bar{P}$ from the *no* region to the signaling region or from the signaling region to *yes*. In some formerly safe districts, then, where even a weak incumbent could avoid strong competition and deterrence played no role, signaling behavior will emerge. In others, where good fundraisers were able to deter quality challengers, strong rival candidates will now enter the race, thus increasing competitiveness.

Within the signaling region, the willingness of type L incumbents to imitate type H's remains unchanged, so the vertical line representing the pooling condition in Figure 5 remains fixed. But the incentives of a challenger to enter any given race have now increased, moving the horizontal line in Figure 5 *upward*. Thus the strategic separation region expands at the expense of pooling. As the electoral environment becomes less favorable to incumbents, top fundraisers come under increased pressure to prove their fundraising skills and separate themselves out from less skilled pretenders. On the other hand, a type L incumbent who was pooling before in the signaling region may move to the *yes* region, thus *decreasing* her fundraising from $\hat{F}_H$ to $\hat{F}_L$. So the overall impact of changing national conditions on fundraising is ambiguous.

Overall, if a rise in anti-incumbent sentiment changes challengers' behavior, it will increase competition. If national forces rise high enough, all districts will become competitive. However, even if entry decisions remain unaltered, patterns of fundraising may still shift; some incumbents will raise more money, and some will raise less. Thus the overall relation between changes in fundraising and changes in the quality of competition is not straightforward, even in the relatively simple case of rising anti-incumbent sentiment.

War Chests

Now assume that the incumbent, in addition to the funds she raises, may also have a war chest, F_0. This is the amount of money in her coffers at the beginning of the election cycle.[19] To incorporate war chests into our model formally, assume that fundraising displays decreasing returns on two levels. First, the functions $F^j_\theta(r)$ are assumed to be strictly increasing in *r*, and the marginal returns are assumed to be strictly decreasing. That is, the first few dollars may be easy to raise, but collecting each additional dollar requires spending more and more resources. Second, funds raised by the incumbent are assumed to provide decreasing returns toward her reelection, so each additional dollar increases her probability of winning less than the previous dollar. The electoral success of the incumbent, then, is proportional to a function $f(F_\theta^j(r))$, where $f' > 0$ and $f'' < 0$.[20]

In equilibrium, a rise in war chests across districts affects electoral competition on two levels. Its most immediate impact is to make the probability of reelection of all incumbents higher since the extra money can be spent on advertising, staff, and so on. More money to spend on campaigns increases the electoral strength of any incumbent. This moves both P_L and P_H to the right in Figure 3. Thus an increase in war chests has the opposite effect from a rise in national factors, making quality challengers less willing to enter any given race.

Another, more subtle effect of incumbents' having larger war chests is that they now have *less* incentive to raise money during the campaign cycle. Given the assumed decreasing marginal impact of spending, incumbents will maximize their electoral strength by spending relatively more resources governing than they did before.[21] If war chests are large enough so that the marginal utility of the last dollar toward reelection is equal to q, then no incumbent will spend any resources chasing money. Rather, she will spend all her time governing.

Consider now the incentives that type L incumbents have to pool with type H's. Analysis presented in the appendix shows that initially the gap between $\hat{F}_L$ and $\hat{F}_H$ may either increase or decrease as war chests rise, but eventually it decreases. This implies that the vertical line in Figure 5 moves to the left, making the simple separation region smaller. Next, consider a challenger who was just indifferent to entering the race if incumbents pool. He will now stay out of the race, since both incumbent types are stronger electorally. So the horizontal line in Figure 5 moves down.

Overall, then, pooling behavior first dominates and eventually takes over the entire signaling region. When both type L and H incumbents have infinite amounts of funds, neither will do any fundraising and imitation is automatic. Thus the infusion of money into the political system discourages strong challengers both directly and indirectly. Not only do war chests increase incumbents' election chances by allowing them to buy more advertising time, send out more mailings, or hire more campaign workers, they also make it easier for weak fundraisers successfully to bluff strong challengers out of entering the race.

Observability

We now weaken the assumption that challengers cannot directly observe anything other than incumbent fundraising. Assume that with some probability β the strong challenger sees the incumbent's type, or, equivalently, the amount of her governing. For instance, the challenger may have access to polling data before he decides whether to enter the race. If there is some probability that an incumbent's type is observed, then any bluff designed to keep strong challengers out of the race may be called.

Greater observability does not affect either P_L or P_H in Figure 3. However, signaling behavior may change. A type L incumbent who was previously willing to raise $\hat{F}_H$ to discourage strong challengers may not be willing to do so now, as there is a chance that her true type will be observed anyway. Thus the vertical line in Figure 5 moves to the right as observability increases. In the limit, the entire region is characterized by simple separating behavior, which is equivalent to the full information outcome. This bears out our intuition that the ability strategically to deceive one's electoral opposition rests on some degree of asymmetric information. Overall, when campaign fundraising becomes more observable relative to governing, this can only *increase* the total funds raised by either incumbent type.

These results allow us to make a few observations on the effect of campaign finance reporting laws on electoral competition. The discussion implies that money's indirect effects—the ability of fundraising to head off strong competition—are inversely related to the observability of incumbent type. The 1974 amendments to the Federal Election Campaign Act, by making money more easily observable than other activities, thereby created an incentive for incumbents to raise large amounts of funds and use them strategically. In addition to the other privileges of office (name recognition, franking, etc.), incumbents now have a strategic advantage that they lacked before—asymmetric information. The overall effects of campaign finance reform are therefore ambiguous. To make money more important in the sense of being carefully monitored also makes it more important in the sense of being a valuable commodity to invest in and use strategically.

CONCLUSION

We conclude by enumerating some implications of the preceding analysis for empirical research in campaign finance and the competitiveness of congressional campaigns. First, our findings suggest that instead of analyzing campaigns as a single process, it may be more fruitful to examine them as a series of separate stages, each with its own logic and strategy. Some previous work has already taken this approach. For instance, Box-Steffensmeier and Lin (1992) fit an Almon polynomial lag structure to incumbent spending, and Green, Robins and Krasno (1991) find some support for the proposition that late spending by incumbents significantly raises their vote margins. Our results imply that fundraising early in the campaign plays a strategic role, different from its role after potential challengers have declared their intentions.

A number of studies have investigated the possibility that incumbents deter qualified challengers but have failed to produce significant results (Bauer and Hibbing 1989; Jacobson 1990a; Krasno and Green 1988). Can the model presented above shed light on these findings? First, there is the question of the appropriate independent variable. The studies mentioned regress challenger quality on total incumbent spending for the entire two-year election cycle. Our model suggests that incumbent fundraising is more appropriate than spending, and in particular money raised early in the campaign is relevant when estimating deterrence.

However, even when the necessary data are collected, direct tests for deterrence may still be difficult to estimate. Our model relates incumbent decisions about resource allocation to challenger quality and election probabilities. The only way to determine whether the hypothesized relationships hold would be accurately to measure the fraction of incumbent resources spent fundraising as opposed to governing. This is a problem typical of signaling models—a true test would require the investigator to observe some factor (governing) that participants themselves (challengers) cannot observe.

Let us then restrict ourselves to the more modest goal of predicting patterns of fundraising and competition across districts. Recall that there are five possible equilibria to the game: the two nonsignaling equilibria in Figure 3 and three signaling equilibria shown in Figure 5. Associated with each of these equilibria is a level of fundraising and challenger quality for each incumbent type; these are illustrated in Figure 6.

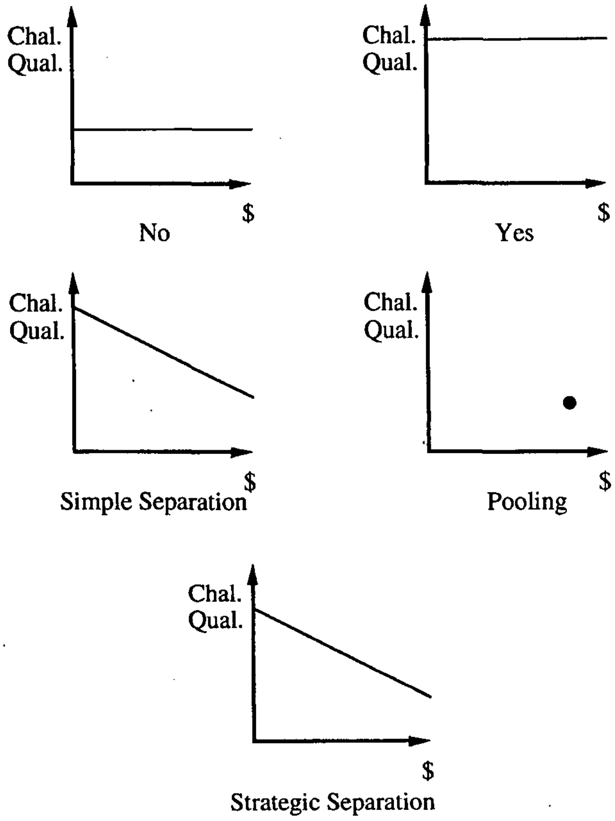
Notice that in only two of the five cases (*simple separation* and *strategic separation*) is there an inverse relationship between incumbent spending and electoral competition. In the other three equilibria, no such relationship exists. Two interpretations of this observation are possible. First, one could note that a data set that mixes all five cases will underestimate the effects of money in deterring quality challengers, because cases where there is no relationship between incumbent fundraising and challenger quality (the *no*, *yes*, and pooling equilibria) are also included in the sample. Furthermore, there seems to be no easy way to separate out the desired cases (*simple* and *strategic separation*) based on easily observable variables, such as district type, incumbent spending, or challenger quality. Thus estimating procedures that include all elections will produce coefficients biased against finding significant deterrent effects.

The second interpretation of Figure 6 emphasizes the need to think carefully about the link between fundraising and competitiveness in congressional elections. Why would we ever assume that greater incumbent fundraising would be associated with lower challenger quality, ceteris paribus? Supposedly because fundraising gives potential challengers information about the incumbent that they did not possess earlier. But not all challengers will base their entry decisions on this information. Specifically, if most challengers fall into the *no* or *yes* regions in Figure 3, then the signaling effects analyzed will occur only in rare circumstances.

The preceding discussion suggests a two-step econometric model. First, a set of variables is needed to estimate a strong challenger's exogenous chances of winning an election, including party registration in the incumbent's district, her vote percent in the previous election, recent scandals, and so on. Second, when the challenger's chances of winning the election are neither "too high" nor "too low,"[22] then early incumbent fundraising should be negatively related to challenger quality. This procedure would be consistent with our earlier theoretical analysis, yielding a more accurate assessment of when incumbents strategically deter challengers and inhibit competitive elections.

FIGURE 6

Equilibrium Patterns of Incumbents Fundraising and the Quality of Challengers Entering the Race



APPENDIX

The following definitions are used in the proofs: $p^e_\theta(F) = \sigma_S(F)p(e_\theta(F), S) + (1 - \sigma_S(F))p(e_\theta(F), W)$ and $p^e_S(F) = \mu(F)p(e_H(F), S) + (1 - \mu(F))p(e_L(F), S)$. Note that condition 2 implies that $\sigma_S(F) > 0 \Rightarrow p^e_S(F) \leq \bar{P}$.

Proof of Theorem 1. Suppose $P_H < \bar{P}$. Then $p(e_\theta(F), S) < \bar{P}$ for all $F \in [0, F_\theta(1)]$ and for $\theta = H, L$, and $\sigma_S(F) = 1$ for all $F \in [0, F_H(1)]$. Hence, $\sigma_\theta(F) > 0 \Leftrightarrow F \in argmax_F p(e_\theta(F), S) = \hat{F}_\theta$ and $\sigma_\theta(\hat{F}_\theta) = 1$.

Suppose $\bar{P} < P_L$. Note that the H incumbent gets his highest possible probability of reelection if and only if $\sigma_H(\hat{F}_H) = 1$ and $\sigma_S(\hat{F}_\theta) = 0$. The following PBE achieves this reelection probability for H: $\sigma_\theta(\hat{F}_\theta) = 1$, $\mu(F) = 0$ for $F \neq \hat{F}_H$, $\mu(\hat{F}_H) = 1$, and $\sigma_S(\hat{F}_\theta) = 0$, with other values of σ_S being anything consistent with 2. $\bar{P} < P_L < p(e_H(\hat{F}_L), S) \Rightarrow \sigma_S(\hat{F}_L) = 0$ in any PBE. Hence, $\sigma_L(\hat{F}_L) = 1$ in any PBE. Q.E.D.

Proof of Theorem 2. Suppose $P_L < \bar{P} < P_H$ and $p(e_L(\hat{F}_H), W) < p(\hat{e}_L, S)$. Then the following is a PBE which gives H his highest possible reelection probability: $\sigma_\theta(\hat{F}_\theta) = 1$, $\sigma_S(\hat{F}_H) = 0$, $\mu(\hat{F}_H) = 1$ and for $F \neq \hat{F}_H$, $\sigma_S(F) = 1$ and $\mu(F) = 0$.

All that remains is to show that in any PBE with $\sigma_H(\hat{F}_H) = 1$, $\sigma_L(\hat{F}_L) = 1$ and $\sigma_S(\hat{F}_L) = 1$. Suppose $\sigma_L(F') > 0$ for some $F' \neq \hat{F}_L$ is part of a PBE with $\sigma_H(\hat{F}_H) = 1$. Then $\mu(F') = 0$ and $p^e_S(F') < \bar{P} \Rightarrow \sigma_S(F') = 1 \Rightarrow p^e_L(F') < p(\hat{e}_L, S) \leq p^e_L(\hat{F}_L)$. By contradiction, $\sigma_L(F') > 0$, hence $\sigma_L(\hat{F}_L) = 1$. Then $\mu(\hat{F}_L) = 0$ and $\sigma_S(\hat{F}_L) = 1$. *Q.E.D.*

Proof of Theorem 3. Suppose $P_L < \bar{P} < P_H$, $p(\hat{e}_L, S) < p(e_L(\hat{F}_H), W)$ and $q < \tilde{q}$. Then the following is a PBE that gives H his highest possible reelection probability: $\sigma_\theta(\hat{F}_H) = 1$, $\sigma_S(\hat{F}_H) = 0$, $\mu(\hat{F}_H) = q$, and for $F \neq \hat{F}_H$, $\sigma_S(F) = 1$, $\mu(F) = 0$.

All that remains is to show that in any PBE with $\sigma_H(\hat{F}_H) = 1$ and $\sigma_S(\hat{F}_H) = 0$, $\sigma_L(\hat{F}_L) = 1$. Suppose $\sigma_L(F') > 0$ for some $F' \neq \hat{F}_L$ is part of such a PBE. Then $\mu(F') = 0$ and $p^e_S(F') < \bar{P} \Rightarrow \sigma_S(F') = 1$. But then $p^e_L(F') < p^e_L(\hat{F}_H)$ and by contradiction $\sigma_L(F') = 0$. Hence, $\sigma_L(\hat{F}_H) = 1$. *Q.E.D*

Proof of Theorem 4. Suppose $P_L < \bar{P} < P_H$, $p(\hat{e}_L, S) < p(e_L(\hat{F}_H), W)$ and $\tilde{q} < q$.

STEP1. Let $(\sigma_L, \sigma_H, \sigma_S, \mu)$ be a PBE. The first step of the proof is to show that in any PBE $\sigma_L(\hat{F}_L) > 0$, $\sigma_H(\hat{F}_L) = 0$, and $\sigma_S(\hat{F}_L) = 1$. Note that $\sigma_H(F) \leq \sigma_L(F) \Rightarrow p^e_S(F) < \bar{P} \Rightarrow \sigma_S(F) = 1$.

Suppose $\sigma_L(\hat{F}_L) \leq \sigma_H(\hat{F}_L)$. If $\sigma_H(\hat{F}_L) = \sigma_L(\hat{F}_L) = 1$, then $p^e_S(\hat{F}_L) < \bar{P}$ and $\sigma_S(\hat{F}_L) = 1$ but then $p^e_H(\hat{F}_H) > p^e_H(\hat{F}_L)$, which contradicts σ_H being part of a PBE. Hence, $\sigma_H(\hat{F}_L) < 1$. Consider an $F \neq \hat{F}_L$ such that $\sigma_L(F) > 0$. However, $\sigma_L(F) > 0 \Rightarrow p^e_L(F) \geq p^e_L(\hat{F}_L) \Rightarrow \sigma_S(F) < 1 \Rightarrow \sigma_L(F) < \sigma_H(F)$. Since this contradicts $\sigma_L(\hat{F}_L) \leq \sigma_H(\hat{F}_L)$, $\sigma_H(\hat{F}_L) < \sigma_L(\hat{F}_L)$.

Finally, $\sigma_H(\hat{F}_L) < \sigma_L(\hat{F}_L) \Rightarrow \sigma_S(\hat{F}_L) = 1 \Rightarrow p^e_H(\hat{F}_L) < p^e_H(\hat{F}_H) \Rightarrow \sigma_H(\hat{F}_L) = 0$.

STEP 2. Define $\bar{F} = max\ \{min\ \{F|p(\hat{e}_L, S) = p(e_L(F), W)\}, min\ \{F|p(e_H(F), W) \leq \bar{P}\}\}$ and $\underline{F} = min\ \{max\ \{F|p(\hat{e}_L, S) = p(e_L(F), W)\}, max\ \{F|p(e_H(F), \bar{W}) \leq \tilde{P}\}\}$. Step 2 is to characterize $\sigma_S(F)$ for any $F \in [\underline{F}, \bar{F}]$ such that $\sigma_H(F) > 0$.

Consider some $F \in (\underline{F}, \bar{F})$ such that $\sigma_H(F) > 0$. Note that $\sigma_H(F) > 0 \Rightarrow F \neq \hat{F}_L$. Suppose $\sigma_L(F) = 0$. Then $p^e_S(F) = p(e_H(F), S) > \bar{P}$ and $\sigma_S(F) = 0$. But then $p^e_L(F) = p(e_L(F), W) > p(\hat{e}_L, S)$ and $\sigma_L(\hat{F}_L) = 0$. By contradiction, $\sigma_L(F) > 0$. For both $\sigma_L(F) > 0$ and $\sigma_L(F) > 0$ it must be that $p^e_L(\hat{F}_L) = p^e_L(F)$, which implies that $\sigma_S(F)$ is given by the following function for $F \in (\underline{F}, fub)$ and $\sigma_H(F) > 0$:

$$\dot{\sigma}_S(F) = \frac{p(e_L(F), W) - p(\hat{e}_L, S)}{p(e_L(F), W) - p(e_L(F), S)}. \qquad \textbf{(A-1)}$$

Suppose $\bar{F} = \min\ \{F|p(\hat{e}_L, S) = p(e_L(F), W)\}$ and that $\sigma_S(\bar{F}) > 0$. Then $p^e_L(\bar{F}) < p(\hat{e}_L, S) \Rightarrow \sigma_L(\bar{F}) = 0 \Rightarrow p^e_S(\bar{F}) < \bar{P} \Rightarrow \sigma_S(\bar{F}) = 0$. By contradiction, $\sigma_S(\bar{F}) = 0$. Note that $\dot{\sigma}_S(\bar{F}) = 0$ in this case. Similarly, $\sigma_S(\underline{F}) = \dot{\sigma}_S(\underline{F}) = 0$ when $\underline{F} = max\ \{F|p(\hat{e}_L, S) = p(e_L(F), W)\}$.

Suppose $\bar{F} = min\ \{F|p(e_H(F), W) \geq \bar{P}\}$. Then $\sigma_H(\bar{F}) > 0 \Rightarrow p(\hat{e}_H, S) \leq p^e_H(\bar{F}) \Rightarrow \sigma_S(\bar{F}) < 1 \Rightarrow p^e_S(\bar{F}) \leq \bar{P} \Rightarrow \sigma_L(\bar{F}) = 0 \Rightarrow \sigma_S(\bar{F}) \geq \sigma_S(\bar{F})$. Similarly, $\sigma_S(\underline{F}) \geq \dot{\sigma}_S(\underline{F})$ when $\underline{F} = max\ \{F|p(e_H(F), W) \leq \bar{P}\}$.

STEP 3. Now consider PBE's that maximize H's reelection probability. Let $(\sigma^*_S, \sigma^*_H, \sigma^*_L, \mu^*)$ be such a PBE. Suppose $\sigma^*_H(F) > 0$ for some $F > \bar{F}$. If $\bar{F} = min\ \{F|p(e_H(F), W) \leq \bar{P}\}$, then $\sigma^*_S(F) = 1$. Note that $e_H(F) < e_H(\bar{F})$ and $\sigma^*_S(\bar{F}) \leq \sigma^*_S(F)$. Hence, the PBE identical to $(\sigma^*_S, \sigma^*_H, \sigma^*_L, \mu^*)$ except that $\hat{\sigma}'_H(\bar{F}) = \sigma^*_H(\bar{F}) + \sigma^*_H(F)$, $\hat{\sigma}'_H(F) = 0$, and $\hat{\sigma}'_S(\bar{F}) = \dot{\sigma}_S(\bar{F})$ gives type H a higher probability of reelection. By contradiction, $\sigma^*_H(F) = 0$ for $\bar{F} < F$. A similar argument for gives $\sigma^*_H(F) = 0$ for $F < \underline{F}$.

H's strategy satisfies the following optimality condition

$$\sigma^*_H(F) > 0 \Leftrightarrow F \in \arg\max_{F \in [\underline{F}, \bar{F}]} \dot{\sigma}_S(F) p(e_H(F), S) + (1 - \dot{\sigma}_S(F)) p(e_H(F), W). \qquad \textbf{(A-2)}$$

Note that $\partial e_H(F)/\partial F \geq 0$ for $F \leq \hat{F}_H$ and that

$$\frac{\partial \dot{\sigma}_S(F)}{\partial F} = (p_1(e_L(F), W) e'_L(F)(p(\hat{e}_L, S) - p(e_L(F), S)) + p_1(e_L(F), S) e'_L(F)(p(e_L(F), W) - p(\hat{e}_L, S)))(p(e_L(F), W) - p(e_L(F), S))^{-2},$$

and then $\partial \dot{\sigma}_S(F)/\partial F < 0$ for $\hat{F}_L \leq F$. Hence, for $F \in [\hat{F}_L, \hat{F}_H]$, the (FOC) for (finalmax) is positive and $\sigma^*_H(F) = 0$.

Consider some $F \in [\underline{F}, \hat{F}_L)$. There exists an F' such that $e_L(F) = e_L(F')$ and $\hat{F}_L < F'$. Note that $e_L(F) = e_L(F') \Rightarrow e_H(F) < e_H(F')$. $\underline{F} < F \Rightarrow [\bar{P} < p(e_H(F), S)$ and $p(\hat{e}_L, S) < p(e_L(F), W)] \Rightarrow [\bar{P} < p(e_H(F'), S)$ and $p(\hat{e}_L, S) < p(e_L(F'), W)] \Rightarrow F' < \bar{F}$. Hence, $e_L(F) = e_L(F') \Rightarrow \dot{\sigma}_S(F) = \sigma_S(F')$. Then $e_H(F) < e_H(F') \Rightarrow \sigma^*_H(F) = 0$. We conclude that $\sigma^*_H(F) > 0 \Rightarrow F > \hat{F}_H$.

Let $\dot{F}_H$ be a solution to A-2. The following is a PBE satisfying our refinement: $\sigma^*_H(\dot{F}_H) = 1$, $\sigma^*_S(\dot{F}_H) = \dot{\sigma}_S(\dot{F}_H)$, $\sigma^*_S(F) = 1$ for $F \neq \dot{F}_H$, $\mu^*(\dot{F}_H) = \tilde{q}$, $\mu^*(F) = 0$ for $F \neq \dot{F}_H$, $\sigma^*_L(\hat{F}_L) = 1 - \sigma^*_L(\dot{F}_H)$, and finally

$$\sigma^*_L(\dot{F}_H) = \left(\frac{q}{1-q}\right)\left(\frac{p(e_H(\dot{F}_H), S) - \bar{P}}{\bar{P} - p(e_L(\dot{F}_H), S)}\right),$$

where the expression for $\sigma^*_L(\dot{F}_H)$ is chosen so that $\mu^*(\dot{F}_H) = \tilde{q}$, hence S is willing to randomize between challenging and not challenging. For simplicity, we assume that $p(\cdot, \cdot)$ is such that $\dot{F}_H$ is the unique maximizer and then any PBE satisfying our refinement will agree with the above strategies on the equilibrium path (i.e., for $F = \dot{F}_H$). *Q.E.D.*

Comparative Statics: National Forces

To formalize the change in national forces we assume that the $p(\cdot, \cdot)$ function in the earlier analysis is shifted by some fixed amount Δ. This shift does not change $\hat{r}_\theta$, $\hat{F}_H$, $\hat{e}_L$, or $\hat{e}_H$. The pooling condition, $p(e_L(\hat{F}_H), W) \geq p(\hat{e}_L, S)$, is unchanged because it depends on the difference between two values of the p function. Hence, national forces do not shift behavior between that in Theorem 2 and that in Theorems 3 or 4.

For concreteness consider a small $\Delta < 0$ so the election environment becomes more favorable for the challenger. While $\bar{P}$ is unchanged because it depends on the preferences of the challenger, P_H and P_L are shifted by Δ since $\hat{r}_\theta$ is unchanged. Thus behavior could shift from that given in Theorem 1 with $\bar{P} < P_L$ to that given in Theorems 2–4 or from that given in Theorems 2–4 to that in Theorem 1 with $P_H < \bar{P}$. In addition, $\bar{q}$ also shifts with Δ as more weight must be placed on $p(\hat{e}_H, S)$ to deter the strong challenger from entering when there is pooling. The lowering of $\bar{q}$ can shift behavior from that given in Theorem 3, where strong challengers never enter, to that in Theorem 4.

Behavior characterized by Theorem 4 is mostly unchanged. Note that the function σ_S defined in equation A-1 is unchanged because it only depends on the difference between two values of the p function. Then, the objective in the maximization problem A-2 is only changed by a constant Δ, hence $\dot{F}_H$ is unchanged. However, σ_L^* is now given by

$$\sigma_L^*(\dot{F}_H) = \frac{q}{1-q}\,\frac{p(e_H(\dot{F}_H), S) - \bar{P} + \Delta}{\bar{P} - p(e_L(\dot{F}_H), S) - \Delta},$$

hence $\sigma_L^*(\dot{F}_H)$ is increasing in Δ.

Comparative Statics: War Chests

We first prove a few results about how various endogenous quantities in the model vary with F_0.

Lemma 5 $\partial P_H/\partial F_0 \geq 0$ *and* $\partial(P_H - P_L)/\partial F_0 \leq 0$.

Proof. For any r, $e_\theta(r)$ and hence $p(e_\theta(r), S)$ is increasing in F_0. Therefore, the maximized value of $p(e_\theta(r), S)$, p_θ, is increasing in F_0. Note that $\hat{r}_\theta$ is given by $f'(F_0 + F_\theta(\hat{r}_\theta) \cdot F'_\theta(\hat{r}_\theta) = g$, unless $f'(F_0) \cdot F'_H(0) < g$ in which case $\hat{r}_\theta = 0$. By assumption $f'(F_0 + F_H(\hat{r}_L) \cdot F'_H(\hat{r}_L) > f'(F_0 + F_L(\hat{r}_L) \cdot F'_L(\hat{r}_L) = g$ and hence $\hat{r}_H \geq \hat{r}_L$. Using the Envelope Theorem,

$$\frac{\partial(P_H - P_L)}{\partial F_0} = f'(F_0 + F_H(\hat{r}_H)) - f'(F_0 + F_L(\hat{r}_L)) \leq 0$$

by the assumption that f is increasing and concave and the fact that $F_L(\hat{r}_L) \leq F_H(\hat{r}_H)$. Q.E.D.

Lemma 6 $\partial\hat{r}_\theta/\partial F_0 \leq 0$

Proof. If $\hat{r}_\theta = 0$ then clearly $\partial\hat{r}_\theta/\partial F_0 = 0$. Suppose $\hat{r}_\theta > 0$. Then it satisfies $f'(F_0 + F_\theta(\hat{r}_\theta) \cdot F'_\theta(\hat{r}_\theta) - g = 0$. The Implicit Function Theorem then yields

$$\frac{\partial\hat{r}_\theta}{\partial F_0} = - \frac{f''(F_0 + F_\theta(\hat{r}_\theta)) \cdot F'_\theta(\hat{r}_\theta))}{f''(F_0 + F_\theta(\hat{r}_\theta)) \cdot (F'_\theta(\hat{r}_\theta))^2 + f'(F_0 + F_\theta(r)) \cdot F''_\theta(\hat{r}_\theta)} < 0$$

by the assumptions that f and F_θ are increasing and concave. Q.E.D.

Comparative Statics: Observability

We assume that with some probability β the challenger is able to see the incumbent's type. Note that the quantities P_L, P_H, $\bar{P}$, $\hat{F}_\theta$ and $\bar{q}$ are all unchanged. The S challenger now has two (behavioral) strategies, one if the incumbent's type is revealed and one if it is not. Let $\sigma_S(F)$ continue to denote the challenger's probability of entry when the incumbent's type is not revealed. Let $\sigma_S(F, \theta)$ denote the challengers probability of entry when the incumbent's type is revealed as θ (and he raises funds F). A *Perfect Bayesian equilibrium* for the extended model is a $(\sigma_S, \sigma_H, \sigma_L, \mu)$ that satisfies:

$$\sigma_\theta(F) > 0 \Leftrightarrow F \in arg\ \max\nolimits_F \beta[(\sigma_S(F, \theta)p(e_\theta(F), S) + (1 - \sigma_S(F, \theta))p(e_\theta(F), W)) + (1 - \beta)(\sigma_S(F)p(e_\theta(F), S) + (1 - \sigma_S(F))p(e_\theta(F), W))],$$

$$\sigma_S(F) \in \arg\max\nolimits_{\sigma_S \in [0,1]}[(1 - \sigma_S)U_S + \sigma_S V(\mu(F)p(e_H(F), S) + (1 - \mu(F))p(e_L(F), S))],$$

$$\sigma_S(F, \theta) \in arg\ \max\nolimits_{\sigma_S \in [0,1]}[(1 - \sigma_S)U_S + \sigma_S V(1 - p(e_\theta(F), S))], \quad \text{and}$$

for all F such that $\sigma_H(F) + \sigma_L(F) > 0$,

$$\mu(F) = \frac{q\sigma_H(F)}{q\sigma_H(F) + (1 - q)\sigma_L(F)}.$$

If $P_L < \bar{P}$, then a strong challenger will always enter when an L-type is revealed; that is, $\sigma_S(F, L) = 1$ for all feasible F. Because with probability β an L-type that attempts to pool incurs the cost of spending an inefficient amount of resources on fundraising without the benefit of facing a weak challenger, there is a new, more restrictive pooling condition:

$$(1 - \beta) \cdot p(e_L(\tilde{r}), W) + \beta \cdot p(e_L(\tilde{r}), S) \geq p(\hat{e}_L, S).$$

Increases in β make the pooling condition harder to satisfy and hence can shift behavior from that in Theorems 3 or 4 to that in Theorem 2, where there are less resources spent on fundraising and L-types always get challenged.

While the characterization of behavior in Theorems 1–3 does not change with β, that in Theorem 4 does. The two following propositions characterize the effect of β on the semipooling equilibria of Theorem 4. First, let $\bar{F} = max\ \{min\ \{F|p(\hat{e}_L, S) = \beta p(e_L(F), S) + (1 - \beta)p(e_L(F), W)\}, min\ \{F|p(e_H(F), W) \leq \bar{P}\}\}$. Note that $\bar{F}$ is an upper bound on $\dot{F}_H$, the amount of funds that an incumbent raises in a semipooling equilibrium. We state without proof the following two propositions.

THEOREM A-1. *Suppose* $P_L < \bar{P} < P_H$, $(p(e_L(\hat{F}_H), W) > p(\hat{e}_L, S)$, *and* $q < \tilde{q}$. *There is predatory fundraising by H types (i.e., they raise an amount* $\dot{F}_H > \hat{F}_H$*) for* $\beta < \bar{\beta}$ *where* $\bar{\beta} \in (0, 1)$ *and simple separation (i.e.,* $F_\theta = \hat{F}_\theta$*) for* $\bar{\beta} \leq \beta$. *In the limit as* $\beta \rightarrow \bar{\beta}^-$, $\dot{F}_H \rightarrow \hat{F}_H$. *Finally,* $\partial\bar{F}/\partial\beta \leq 0$ *for* $\beta < \bar{\beta}$.

The next theorem considers the effect of shifts in β when $\dot{F}_H < \bar{F}$.

THEOREM A-2. *Suppose* $P_L < \bar{P} < P_H$, $p(e_L(\hat{F}_H), W) > p(\hat{e}_L, S)$, $q < \tilde{q}$, *and* $\beta < \bar{\beta}$. *If* $\dot{F}_H < \bar{F}$, *then* $\partial\dot{F}_H/\partial\beta$ *may be either positive or negative. If* $p(e, \phi) = e + \Delta_\phi$ *where* $\Delta_W > \Delta_S$, *then* $\partial\dot{F}_H/\partial\beta = 0$, $\partial\sigma_L(\dot{F}_H)/\partial\beta = 0$ *and* $\partial\sigma_S(\dot{F}_H)/\partial\beta < 0$.

Notes

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1. See, e.g., Ansolebehere 1990; Green and Krasno 1988; and Jacobson 1990a. Useful summaries of this literature are available in Sorauf 1992 and Fowler 1993.

2. For an overview of this theoretical literature and a discussion of service-induced contributions, see Morton and Cameron 1992.

3. It could be argued that in the absence of these FEC regulations, candidates could still signal their fundraising to potential challengers. However, this would require establishing a credible verification system at some cost to the incumbent. Commission regulations provide such a verification system at taxpayers' expense.

4. This trade-off is explored at length by Fenno (1978) in his classic work, *Home Style*. For instance, Fenno notes: "The job of a congressman requires that some things be done in Washington and others be done in the district. At the least, legislation is passed in one place and elections occur in the other. The allocative problem, therefore, comes with the job" (p. 33). For models of legislative decisions about time and resource allocation, see also Denzau and Munger 1986 and Hall 1989.

5. This efficiency might be interpreted in a variety of ways. First, one incumbent might have direct access to political action committee money, while for others this money might come at the cost of either a great amount of time spent raising it or the necessity of casting votes that are unappealing to her constituents. Some candidates might have a network established from which they can raise significant amounts with very little effort, and others might simply have superior interpersonal skills that help attract contributions.

6. Note that in general, returns to fundraising will vary by district, so a bad fundraiser from New York City might in fact be more efficient than a good fundraiser from Montana.

7. The log of incumbent spending produces a better fit in explaining incumbent vote than does unadjusted spending, thus supporting the idea that spending displays decreasing returns to scale. Green and Krasno 1988, 892; Jacobson 1990a, 54.

8. The restrictiveness of this linearity assumption is primarily for analytical convenience; we could have assumed only that there is a unique point where the marginal benefits of fundraising and governing intersect.

9. That is, we assume that e is decreasing in r beyond some point.

10. A legislator will budget her time so that the marginal return in terms of reelection chances from governing just equals that from fundraising $(F'_\theta(\hat{r}_\theta)) = g)$. After the type L legislators have reached this point, type H's can still earn more than the constant return on governing by raising more funds. Thus, they are more effective at the point where they reach their optimal fundraising level, ending up with both more money and greater reelection chances.

11. We make one simplifying assumption, namely that $\hat{F}_H < F_L(1)$, so that it is at least feasible for the L-type incumbent to raise the optimal level of funds for the H-type.

12. In the limit, the incumbent may be unopposed. Token opposition, possibly from a challenger entering the race mainly for reasons other than the possibility of being elected, approximates no competition in many cases. Banks and Kiewiet (1989) prove that weak challengers will rationally enter races even when the incumbent is quite strong, since in those years they have a greater probability of surviving the primary.

13. Fudenberg and Tirole provide a useful survey of the issues involved (1991, chaps. 8, 11).

14. This type of reasoning is similar to that used by Cho and Kreps (1987) in their definition of the "intuitive criterion," and by Farrell (1988) in his discussion of "neologism-proof" equilibria.

15. This would be true as long as the incumbent types are separating at any two different amounts of funds raised, not just $\hat{F}_L$ and $\hat{F}_H$.

16. Note that pooling does not require incumbents across all districts to raise the same amount of money. If each district has distinct returns to governing (different values of g^j), then pooling applies only to the possible fundraising levels within any particular district.

17. Of course, all players' actions are strategic in this analysis. Thus the emphasis in the term *strategic separation* should be placed on the second word. Here, type H incumbents take strategic actions that *separate* them from type L incumbents.

18. For simplicity, Theorem 4 focuses on equilibria where the H type uses a pure strategy. Further restrictions on $p(\cdot, \cdot)$ are required to assure that there are not equilibria that satisfy our refinement in which incumbents mix over different levels of funds raised. However, the proof shows that any level of funds raised by the H type is greater than $\hat{F}_H$.

19. The term *war chest* has been used in two ways in the campaign finance literature. It can mean the money an incumbent has to start the campaign, or it can refer to the money a candidate amasses during the campaign. We adopt the former definition here; the latter is subsumed in our fundraising function $F_\theta(r)$.

20. We assume that $f'(F_0^j + F_H^j(r)) \cdot F'_H(r) > f'(F_0^j + F_L^j(r)) \cdot F'_L(r)$, so the H-type still gets a higher marginal benefit from a given amount of resources spent. We also assume that $\lim_{F_0 \rightarrow \infty} f'(F_0^j) \cdot F'_H(0) < g$, so that for sufficiently large war chests the incumbent spends all her time governing.

21. This point is similar to the one made in Gilligan and Krehbiel 1992. The authors equate high electoral margins with lower opportunity costs of specialization and argue that as legislators become more secure electorally, they are more likely to spend resources acquiring policy expertise.

22. These cut points would be estimated via a maximum likelihood procedure along with the other parameters of interest.

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THE RESPONSIVE VOTER: CAMPAIGN INFORMATION AND THE DYNAMICS OF CANDIDATE EVALUATION

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We find strong support for an on-line model of the candidate evaluation process that in contrast to memory-based models shows that citizens are responsive to campaign information, adjusting their overall evaluation of the candidates in response to their immediate assessment of campaign messages and events. Over time people forget most of the campaign information they are exposed to but are nonetheless able to later recollect their summary affective evaluation of candidates which they then use to inform their preferences and vote choice. These findings have substantive, methodological, and normative implications for the study of electoral behavior. Substantively, we show how campaign information affects voting behavior. Methodologically, we demonstrate the need to measure directly what campaign information people actually attend to over the course of a campaign and show that after controling for the individual's on-line assessment of campaign messages, National Election Study–type recall measures prove to be spurious as explanatory variables. Finally, we draw normative implications for democratic theory of on-line processing, concluding that citizens appear to be far more responsive to campaign messages than conventional recall models suggest.

How much of what kinds of campaign information citizens can recollect about parties, candidates, and issues at the time a decision is called for is the keystone of virtually all contemporary models of individual political behavior and the cornerstone as well for our assessment of the competence of the democratic citizen. The underlying assumption throughout the literature of individual political behavior is that citizens have a storehouse of political information they can draw on to inform their political behavior, whether in reply to survey questions, in political conversations, or in the voting booth.

In the study of electoral behavior, for example, our very best models of issue voting treat candidate evaluations as a function of the respondent's recall of self and candidate proximities on the issue scales (Enelow and Hinich 1984), while our discipline's most predictive model (arguably, Kelley and Mirer's [1974] "simple act of voting") shows a strong correlation of candidate evaluation with the voter's net recollection of likes and dislikes. Our models of political behavior outside the polling booth are often memory-based, too (see Price and Zaller 1993), as are our normative expectations about the scope and depth of political knowledge that the democratic citizen can and should bring to mind to act purposefully (see Barber 1973; Berelson 1952; Berelson, Lazarsfeld, and McPhee 1954; Kessel 1988; Neuman 1986; Smith 1989; Weissberg 1974; also see Hanson and Marcus 1993).

All well and good were it not for a most troublesome incongruity—to wit, citizens do not measure up to model specifications. Fifty years of survey data portray a rather bleak picture of the American citizen as one who is not nearly as aware as our models suppose and less informed than normative theories proscribe (see Hanson and Marcus 1993; Kinder and Sears 1985). More often than not, when voters are asked on election day about parties, candidates, and issues they are found to be ill informed—the majority of respondents unable, for example, to cite more than two or three likes or dislikes to the National Election Study (NES) open-ended questions and probes (see Smith 1989). And for many of these "good" citizens their responses are, to put it kindly, "diffuse" (Gant and Davis 1984).

That citizens often cannot remember many details of election campaigns is not being contested here, as the evidence is overwhelming. However, we challenge the longstanding assumption that the citizen's failure to recall campaign events is necessarily or even primarily a function of political inattentiveness, political ignorance, or (worse yet) irrationality. In contrast to conventional wisdom, we do not interpret the failure of J.Q. Public to recollect basic political facts (Delli Carpini and Keeter 1991; Erskine 1963), or recognize ideological language (Converse 1964), or recall a candidate's characteristics (Abramowitz 1975), or even remember candidate names (Neuman 1986) as a sure sign of an uninformed citizenry acting "in the dark." Rather, we propose here and will test empirically a "bounded rationality" model of candidate evaluation and vote choice that turns the memory-based assumption on its head in arguing that citizens can be (and in fact typically are) responsive to campaign information—their overall evaluations reflecting their assessment of all the information they are exposed to—but are unable, for good reasons, to recollect accurately the considerations that entered into their evaluations.

Recall and the Enigma of the Informed Voter

We propose and find empirical support for a solution to two paradoxes that plague contemporary studies of electoral behavior: (1) the paradox of the informed voter, whereby voters (in the aggregate) oftentimes appear to be choosing the "right" candidate and supporting the "correct" issue stands but apparently without the conceptual or factual wherewithal to make such informed judgments (Page and Shapiro 1992), and (2) the paradoxical discrepancy between survey and experimental research findings, where we typically find in survey research a strong, positive correlation between the mix of pros and cons in memory and the direction and strength of evaluation (Kelley 1983) but can rarely find a direct memory-to-judgment link under experimental conditions (Lodge, McGraw, and Stroh 1989).

Consider two studies that we think point us in the right direction. The first, an experiment by Watts and McGuire (1964), looked at people's recollection of the arguments of a persuasive message over a six-week period. Watts and McGuire found (as do most replications; see Anderson and Hubert 1963; Hastie and Park 1986) *no* systematic effect of recall on the persistence of induced opinion change. Apparently, messages do not lose their effect on people's opinions once the content of the messages is forgotten. Thus it is that voters may be strongly affected by campaign information without such responsiveness being captured in their recollections. This finding is commonplace under laboratory conditions where exposure to the content of messages is known and the researcher can directly compare what information subjects are actually exposed to and what they can later recall (Hastie and Pennington 1989; Lichtenstein and Srull 1987).

Consider next Graber's conclusion to her longitudinal study of the 1976 presidential campaign that "the fact that so little specific information can be recalled from a [news] story does not mean that no learning has taken place. The information base from which conclusions are drawn may be forgotten, while the conclusions are still retained. This seems to happen routinely. Voting choices, for instance, often match approval of a candidate's positions even when voters cannot recall the candidate's positions or the specifics of the policy. In such cases, media facts apparently have been converted into politically significant feelings and attitudes and the facts themselves forgotten" (Graber 1984, 73). Again, campaign messages and events can exert an influence on voters' attitudes independently of their recall of the considerations that entered into their judgments, apparently long after the triggering campaign events have been forgotten.

The issue, then, is not how many campaign events or candidate positions voters can recall accurately at the time they are called on to express a belief or preference but, instead, how *responsive* the citizens' overall evaluation and vote choice is to the political information they considered throughout the campaign. More specifically, how much of what kinds of campaign information do citizens integrate into their summary assessments of candidates and how well does their overall assessment of the candidates reflect their evaluation of all the campaign information they attended to?

As every standard text in cognitive psychology points out (Eysenck and Keane 1990; Lachman, Lachman, and Butterfield 1979), a basic limitation of human information processing is the fallibility of memory: people forget . . . a lot. Memory fades over time and while this process may be faster for some people than others and for some types of information than for others, the inevitable outcome is that people's recollections decrease in number and get fuzzier over time. Thus it may well be that the citizen's inability to recall basic political facts reflects limitations of the human mind rather than unsophistication of the democratic citizen. Our criticism, then, is not just directed against memory-based models of the vote choice but, more broadly, challenges the memory-based assumption underlying contemporary analyses of political behavior in general and, still more broadly, the negative normative conclusions routinely drawn from the citizenry's failure to recall campaign events.

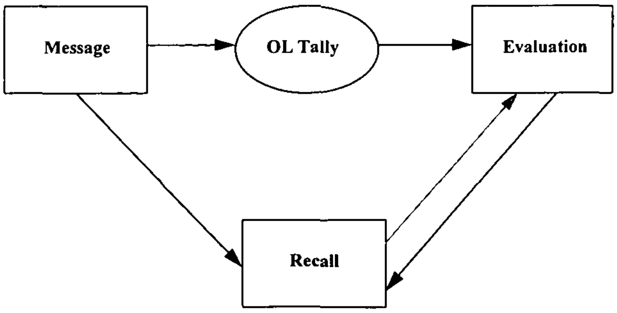
How can information exert an enduring influence on people's opinions despite their inability to retrieve these considerations from memory? One suggestion is that the message-judgment relationship is mediated by a mechanism other than recall. Indeed, Graber (1984) hints at some such process when she claims that voters draw conclusions from campaign information while forgetting its contents. In essence, what she describes is what has come to be called "the on-line model of information processing," which traces its origins to social psychology (Anderson 1991; Hastie and Park 1986; Hastie and Pennington 1989) and has recently been extended to the domain of political impression formation (Lodge, McGraw, and Stroh 1989; Mackie and Anuncion 1990).

A Model of Voter Responsiveness

A schematic depiction of our expectations for an on-line (OL) model of the responsive voter is presented in Figure 1. According to this model (see Lodge and Stroh 1993), when one's goal is to form an overall impression of some person, place, or thing, most people most of the time appear to act as bounded rationalists in simplifying the judgmental process by drawing politically relevant conclusions from the information at the very moment they encounter it and then and there, when the message is before their eyes, so to speak, spontaneously culling the affective value from each specific candidate message, and immediately integrating these assessments into a "running tally" that holds the individual's

FIGURE 1

On-Line Candidate Evaluation Model



summary evaluation of the candidate (as depicted by the solid arrow in Figure 1 from Candidate Message to OL Tally). This OL tally, sometimes called an "affect-integrator," is then immediately stored in long-term memory and the considerations that contributed to the evaluation are quickly forgotten (hence the weak—grey—path from Recall to Evaluation). Then, later, when called on to make a judgment—to a pollster or in the voting booth—it is this summary impression, not recollections of the original campaign information, that comes to mind to guide the decision (as anticipated in Figure 1 by the strong, direct connection from OL Tally to Evaluation). From this OL perspective, "responsive voters" will decrease their general evaluation of a candidate when confronted with negative information and increase their candidate evaluations when made aware of information that they judge to be positive.[1]

Essentially, then, the OL model posits a strong indirect effect of attention to campaign messages on judgment, mediated by the OL Tally. Moreover, there is not necessarily a strong relationship between the pros and cons that actually enter into the evaluation at the time of exposure and one's recollections of these considerations. The forgetting of the facts—long the bane of memory-based models—is of little consequence to the OL model, since the affective value of campaign messages has already been integrated into the evaluation and vote decision. Thus it is that voters can oftentimes tell you how much they like one candidate or another but not be able to tell you many of their reasons why. Or else (this being the reason, we think, for the strong correlation between recall and evaluation in most surveys like the NES), having forgotten many of the considerations that entered into their overall evaluation, when called on to report the *whys* and *wherefores* for their evaluation, respondents are prone to search memory for supporting "evidence" and dredge up commonsensical rationalizations for their preferences (as suggested by the solid arrow from Evaluation to Recall in Figure 1; see Brody and Page 1972; Kunda 1990; Lau 1982; Nisbett and Ross 1980; Rahn, Krosnick, and Breuning 1994; Wilson and Schooler 1991). From this perspective, what citizens are likely to recall about the candidates is their global affective assessment of them, not the specific considerations that actually entered into the evaluation. At best, the citizen's recollections will represent a biased sampling of the actual causal determinants of the candidate evaluation (Anderson and Hubert 1963). At worst (this, we think, being a plausible explanation for marked differences between survey and experimental findings), the correlation between memory and judgment is the result of reversed causality, the causal arrow often going from evaluation to memory.

If the OL model is correct, citizens can be attentive and still not recall much if any campaign information. This model of political information processing has serious implications for how we view the connection between campaign events and political judgment. Inherent in the logic of on-line information processing is a radically different notion of what an informed citizenry means. Rather than measuring the citizen by what he or she can recall from the campaign, as memory-based models tend to do (see Kessel 1988), we ask the normatively more interesting question whether citizens actually incorporate campaign information in political judgment and choice. If, on attending to campaign stimuli, citizens were to first establish and thereafter update their OL tally in response to new information and finally bring this summary evaluation to bear on political judgments, we would claim that the citizens are responsive to and informed by campaign information.[2] On the other hand (this being the alternative hypothesis to be tested in this study), if the citizen's political judgments were found to be but tangentially related to the campaign information he or she was exposed to, then we too would be forced to say that citizens are uninformed.

EXPERIMENTAL DESIGN AND PROCEDURE

This study was designed to provide direct comparisons of the OL versus memory-based models of candidate evaluation *over time*, so that a *dynamic* picture of the candidate evaluation process may be obtained. The study addresses two critical questions: (1) Does the relationship typically go from *message to recall memory to evaluation* (as memory-based models suggest), or is the candidate evaluation process more aptly represented by the path from *message (to OL tally) to evaluation*? and (2) How strongly are message and evaluation related, that is, *how responsive are citizens to campaign information*?

Any attempt to disentangle the impact of memory on political judgment must necessarily measure the actual content of candidate messages and look at recall over the more realistic time spans of electoral campaigns. In our view, past research, both experimental and survey, has generally succeeded in satisfying one of these requirements but not both. The strength of the experimental approach is that it allows us to isolate memory effects by controlling the content of messages to which subjects are exposed, thereby getting a direct test of the message-judgment relationship. Unfortunately, an all-too-typical shortcoming of this approach—ours among them (Lodge, McGraw, and Stroh 1989)—has been to look at the relationship between message and recall after only short, single-session time intervals on the order of five or ten minutes, rather than the more relevant day-, week-, and month-long time spans of campaigns. Conversely, in survey research longer time delays are more common (with most NES data based on postelection interviews), but here there is no way to determine what information citizens were actually exposed to (Price and Zaller 1990). This forces researchers to rely on one or another self-reported measure of attention to campaign messages and leaves us with no measure of the order, timing, content, or complexity of the messages. As a result, it is impossible to obtain an accurate estimate of the impact of memory on political judgment.

In the present study we combine the strict control over exposure to campaign messages available to experimenters with the realistic time spans found in surveys by introducing time delays of 1 to 31 days in a $t_1 \times t_2$ design to test the memory-judgment relationship over time. In addition, we obtained a finer-grained analysis of the message-judgment and memory-judgment relationships by considering two factors that are known to influence their size. First, we manipulated the partisan consistency of the candidate profiles that we presented to our subjects, so as to examine our "responsive voter" model when a candidate is somewhat at odds with one's partisan expectations. Second, for half the subjects, we set up a condition in which we tested the OL model against an idealized memory-based model by encouraging subjects to think more deeply about the candidates and their policy positions, hereby mimicking a basic condition of rational voter models.

Sample

A nonrandom sample of 356 nonstudent adults from Long Island, New York, was interviewed by trained college students in the spring of 1991. Fifty percent of the subjects were male, 92% white, and 71% were college graduates or had had some college. The samplewide median income was $25,000. More pertinent for our analyses, the sample was about equally split in terms of their partisan affiliation, with 27% of the subjects reporting identification with the Republican party, 33% with the Democratic party, and the remainder calling themselves independents or reporting no party affiliation.

Experimental Stages

The experiment consisted of three core stages. First, subjects completed a self-administered questionnaire under supervision of our interviewers. The questionnaire asked subjects to evaluate information that would show up in the candidate messages that the subjects read later in this stage of the experiment. Second, a variable $t_1 \times t_2$ delay of 1 to 31 days followed exposure to the campaign information. Finally, a telephone interview took place in which recall data and candidate evaluations were obtained. Of the 356 individuals who completed the t_1 questionnaire, 211 were successfully contacted for the t_2 call-back portion of the experiment, making for a panel attrition rate of 40%. More specifically, the experiment consisted of the following parts.

Stage 1(a): Political Beliefs and Preferences. The subjects were recruited to participate in a study that had the ostensible aim of evaluating two competing candidates running for the U.S. Congress. In part (a), subjects read a randomized series of 66 information items that described various policy statements (e.g., "A candidate who supports the death penalty for drug-related murder") or candidate characteristics (e.g., "A candidate who served in the U.S. Navy (1963–1967)"). Subjects were asked to evaluate each policy statement and candidate characteristic on a five-point scale, ranging from "very positive" to "very negative." For each policy statement, subjects were also asked to indicate whether Democrats or Republicans would be likely to support it, this to tap partisan stereotypes in our subjects. This information was later used to assess the prototypicality of the candidates to which we exposed our subjects.

Stage 1(b): Attitude Survey. After evaluating the candidate characteristics and policy statements, subjects were asked for basic demographic information, as well as their party identification, ideological persuasion, political interest and efficacy, and knowledge of contemporary political figures (measured by the recognition of each politico's party affiliation). This section of the survey concluded with a series of questions concerning the importance of the general policy domains (e.g., abortion, crime, the federal deficit) on which the candidates would later make specific policy recommendations. Note that, in addition to providing data on basic subject characteristics, this step also served as a distractor task between the earlier assessment of information and the upcoming exposure to the campaign messages.

Stage 1(c): Information Exposure (Campaign Fact Sheet). After completing the attitude survey, subjects read a one-page facsimile of a campaign fact sheet (reproduced in Appendix A) which presented the party affiliation, seven-issue positions, and nine personal attributes of two congressional candidates (Dave Wagner (R) and Tom Messinger (D), both hypothetical). The format of the fact sheet was modeled on the way candidates are summarily compared on election eve in the local area newspaper, *Newsday.*

The fact sheet included a within-subject manipulation of the candidate profiles. Whereas the Republican candidate adopted consistently Republican stands on the issues (as established in pretests), two of the seven policy stands of the Democrat were somewhat atypical of Democratic candidates. For instance, unlike the prototypical Democrat (but like Tsongas in the 1991–92 primaries) our candidate sought to cut programs to balance the federal budget and (like Clinton) supported the death penalty. As such, he "issue-trespassed" (Norpoth and Buchanan 1992), adopting positions along Republican lines. Thus the candidates were not mirror images of one another: the Democratic candidate deviated on two of seven issues from New Yorkers' stereotypic image of a Democrat. A manipulation check, based on the typicality ratings of the issues for each party in part (a), confirmed the slightly skewed portrait of the Democratic candidate and the prototypicality of the Republican. Our hypothesis is that the somewhat incongruent profile of the Democrat makes him a more complex (and, we think, more realistic) candidate. As a result, the evaluative process should be more difficult for the Democrat than for the Republican candidate, since subjects are denied a simple stereotypic assessment and can less easily rationalize a set of partisan-rooted recollections.

Stage 1(d): Processing-Depth Manipulation. At this point, after having read the candidate fact sheet, half of the subjects (selected randomly) were thanked and dismissed. The remaining 178 subjects received a series of 50+ questions designed to prod them into thinking more about the candidates and their issue stands. These subjects were asked to evaluate the two candidates on a five-point scale that ranged from "very positive" to "very negative," to list their likes and dislikes of each candidate, render 24 trait inferences (Kinder 1986), and make a vote choice. Depending on the number of arguments each subject gave for supporting or opposing a candidate, anywhere from 50 to 62 supplemental questions were being answered.

In having these subjects mull over the campaign information and reconsider its implications, we should expect a deeper level of information processing, which should in turn bolster the memory traces for the campaign message. As a consequence, there should be less forgetting (Craik and Lockhart 1972) and, if memory-based processing works as advertised, a stronger recall-judgment relationship. Conversely, the OL model would predict *minimal differences* between the simple and depth-processing conditions and a message-evaluation effect stronger than the recall-evaluation effect in *both* conditions, as we posit that when forming general impressions of people, places, and things, people quickly forget the considerations that entered into an evaluation once the affective value has been culled from the message.

While the depth-processing manipulation provides a strong test of the OL model, it was also intended to parallel two different types of citizenship that have been described in the literature. When asked explicitly to think about the campaign information and mull over their reasons for supporting one candidate over the other, we suppose subjects took on the role of the citizen that normative theorists have sketched—an individual who processes campaign information in depth to arrive at an informed decision reflecting serious consideration of the pros and cons of a candidate's issues stands. Such in-depth processing is the most elementary requirement for rational citizenship (Barber 1973). In contrast, subjects who were not prodded by the 50+ questions probably did not engage in in-depth processing. Consequently, their behavior should correspond more closely to what is typically described in empirical analyses of citizenship as a citizenry that pays only cursory attention to politics (Berelson 1952; Berelson, Lazarsfeld and McPhee 1954; Lippmann 1991; also see Kinder and Sears 1985) and relies on factors outside of the campaign to forge candidate evaluations (Berelson, Lazarsfeld, and McPhee 1954; Campbell et al. 1960).

Stage 2: Delay. Upon completion of the stage 1 questionnaire all subjects were randomly assigned to one of the 31 $t_1 - t_2$ delays. All subjects were told they would be reinterviewed and left their number for a call-back. When the set time had expired, subjects were contacted by a new interviewer. If contact could not be established, the subject was rotated to the next day and another subject was contacted in lieu. One consequence of this procedure is that the number of call-backs for the different delays is not uniform, as it turned out to be easier to contact people on weekdays than weekends. Nonetheless, we were able to obtain t_2 observations for all possible delays.

Stage 3: Subject Recall and Candidate Evaluation. In the call-back telephone interview, subjects were asked to give their evaluations of the two candidates on a five-point scale, ranging from "very positive" to "very negative." For the subjects who had not been asked the 50+ questions in stage 1(d) this was the first time they offered an evaluation of the candidates. After voicing an evaluation of each candidate, all subjects were asked if there was anything positive or negative they could remember about each candidate, with each query followed by an "Anything else?" probe. The format of the recall questions was essentially that used in the NES, except that we gave our subjects the names of the candidates if they were unable to recall which candidate was the Democrat and which one the Republican.

Stimuli: Campaign Fact Sheet

The campaign information that our subjects received was rather rich in content and tried to mimic real-life campaign stimuli. When presenting their issue positions political candidates (and newspaper summaries) often make a general policy statement, which is then embellished with specific recommendations that moderate the policy statement or elaborate on it. For instance, a political candidate might argue that he "is opposed to abortion" but then qualify his stand by adding "except in the case of rape and incest." Or a politician might say that she is "tough on crime" and then elaborate her policy statement with a call for mandatory jail sentences for, say, drug king-pins.

We call the core policy statements *gists* and the details *specifiers*. All 14 issue positions in the two-candidate fact sheet were *complex statements* combining a gist with one or more specifiers. For instance, the position of the Democratic candidate on the issue of crime and drugs reads as follows (the gist always presented as the first sentence or phrase of the policy statement, with one or more programmatic specifiers following): "Tom Messinger supports federal laws which crack-down on drugs and crime. He favors the death penalty for murderers, mandatory sentences for drug king-pins, and a mandatory waiting period and background check to purchase a firearm. Tom also supports treatment programs for drug addicts.[9]

An important question to be resolved in the upcoming analyses is whether candidate evaluations are responsive just to the issue gists or whether evaluations reflect subjects' assessment of the candidates' complex statements—the gist and specifiers. Recall that in stage 1(a) we had all subjects rate the gists *and* complex policy statements separately so that we could analyze the impact on evaluations of the full message as well as the impact of the gists only. Our OL model posits that the OL tally is responsive to the gists *and* specifiers of campaign messages but that, over time, first the specifiers and then the gists will be forgotten. If so, the OL model would portray the voter as responsive, while a memory-based model would picture the very same voters as unaware and ignorant.

RESULTS

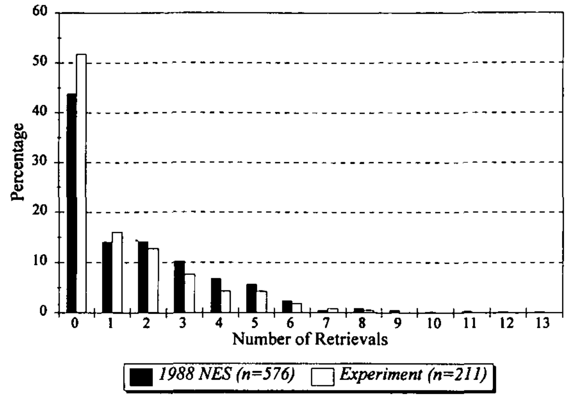
What do the experimental data tell us about memory for campaign facts and their role in the candidate evaluation process? And what do the data tell us about the direct impact of the campaign message on candidate evaluation, that is, the impact that is not mediated by campaign recollections? To determine the effects of campaign messages and recall of these messages, we propose to look first at memory decay—how much of what kinds of information is forgotten over time and how quickly?—then analyze the predictive power of message and recall on candidate evaluation, and conclude with a causal analysis of the candidate evaluation process that is patterned after Figure 1.

The Nonpersistence of Memory

When we look at how much our subjects could recall from the campaign messages that they had received, our results are entirely consistent with the dominant finding in studies of individual political behavior (Delli Carpini and Keeter 1991; Erskine 1963; Neuman 1986; Smith 1989): *citizens forget . . . a lot*, with about 54% of the subjects unable to recollect a single issue that either of the candidates had addressed.[3] The modal number of recalls for the issue gists was zero, with 60.7% of the subjects unable to recall a gist—correct or incorrect[4]—for the Republican candidate and 62.1% unable to produce gist recollections for the Democrat. Recall for the specifiers was worse still, with close to 80% of the subjects not able to recollect even one specifier (again, correctly or incorrectly) for either of the candidates.[5]

FIGURE 2

Retrieval in 2-Candidate Elections



This dismal level of recall of campaign information by our subjects is *not* different from what researchers typically find in surveys about real-life candidates. For instance, when we contrast the recalls in our experiment with those for congressional candidates in the 1988 NES, we find a striking congruence. As Figure 2 demonstrates, in both samples the modal number of recalls is zero, with very few people providing more than two recollections of the campaign. Indeed, the recall patterns are so similar that there is no statistical difference between them ($\chi^2 = 12.497$, d.f. = 12, *ns*).

Voters, then, appear to forget much of the campaign information to which they were exposed. This, however, is not to say that the level of forgetting is constant: some voters under certain conditions will forget more than others. Among the factors that seem to be most instrumental in determining the level of memory decay are political sophistication (Fiske, Kinder, and Larter 1983; McGraw and Pinney 1990) and the depth of information processing (Craik and Lockhart 1972).

We tested these effects using the between-subjects depth-of-processing manipulation of stage 1 of the experiment, as well as a median split of the Subjects in terms of their political knowledge—a standard measure of political sophistication (Zaller 1990).[6] Because recall is also influenced by the time delay after exposure, delay was introduced in the analysis as a covariate. We also included age as a covariate, because past research has shown (and as the senior author can attest) that recall performance deteriorates with age. Separate analyses were performed for the complex statements and for the gists alone.

Repeated-measures ANCOVAs show that the covariates had the strongest impact on the combined[7] gist recalls for both candidates (F[2, 205] = 11.64, p = .000). The strongest effect came from delay (b = −.031, β = −.289, p = .000), with age exerting a somewhat smaller influence (b = −.011, β = −.194, p = .005). The effect of the covariates on recalls of the complex statements for both candidates were somewhat weaker (F[2, 205] = 4.57, p = .011), in large part because the impact of delay was much smaller. Its effect (b = −.012, β = −.159, p = .023) was rivaled by the effect of age (b = −.012, β = −.165, p = .021).

For the gists, there was also a significant main effect for the depth-of-processing manipulation (F[1, 205] = 4.23, p = .041), with subjects exposed to the 50+ follow-up questions to the campaign fact sheet able, as predicted, to recall more than other subjects (the respective means being 1.06 and .74 information items, respectively). No significant effect of the depth-of-processing manipulation was found for the complex statements. Apparently, recall of campaign specifics is not enhanced much by asking people to think about and elaborate on the campaign information.

A more systematic effect on recall was exerted by the structural differences between subjects in their knowledge structures. For the complex statements, one's level of general political knowledge exerted a highly significant effect (F[1, 205] = 5.87, p = .016), with subjects high (above the median) in knowledge showing twice the number of recalls of subjects low in knowledge (the respective means are .57 versus .29 recalls for the two candidates). Political knowledge also had an effect on gist recall. Although this effect was only marginally significant (F[1, 205] = 2.93, p = .088), its size was similar to that found for the depth-of-processing manipulation.

While these results are informative about the factors that affect recall, their real significance is that memory for campaign information was universally weak. Conceivably, subjects could recall up to 14 issue gists but not even the extensive battery of questions and probes that made up the depth-of-processing manipulation brought recall anywhere near this upper limit. Similarly, while political sophisticates recalled twice the number of issues of nonsophisticates, their average recall was still well below a single information item. In conclusion, the recall of campaign information appears dismal even under the best of circumstances, that is, when the information is processed by knowledgeable citizens or is processed in depth.

With recall being so low, an important question becomes what it is that is being recalled. If citizens fall short of possessing a storehouse of campaign information, does what is being recalled at least provide an adequate basis for sound political judgment? Our data, like that of the NES, are not very reassuring of the quality of voters' recollections. First, issues were not recalled because of their salience to our subjects: the null hypothesis that the message is, on average, as important as the recalled information or more important could not be rejected.[8] Thus, recall appears to be a fairly indiscriminate process, which may easily result in the recollection of issues that are not of great importance to the voter. Second, while the evaluation of the message and the recalled information were positively correlated, the association was rather weak: r = .268 for the Republican and r = .274 for the Democrat. Hence, what voters recall is, in terms of issue evaluations, not necessarily a fair sample of the full message.

In the light of this evidence, a vote choice that is informed by the campaign issues seems to be out of the question *if* we assume that candidate evaluation is memory-based. Fully 71% of our subjects would not be able to make an informed choice, either because they could not recall anything about the two candidates or because they could only provide recalls for a single candidate and could hence not make issue-based candidate comparisons. For the subjects who could recall something (anything, right or wrong) about both candidates (n = 61)—and could conceivably thereupon make comparisons—voting would be based on no more than two issues about half of the time.

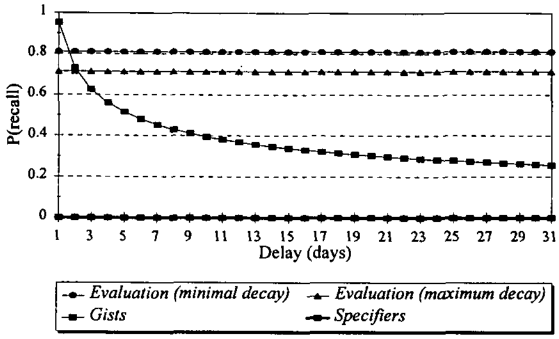
The memorial wherewithal of voters can be questioned even further if we consider the dynamics of memory, that is, how recollections change over time. In the ANCOVAs we postulated recall to be a linear function of delay. As a first approximation this is not a bad assumption, but better recall models are available. A model that has received particularly strong support in the psychological literature is the *power decay model*, which assumes recalls to decline over time through a power function, such that recollections of campaign messages fade quickly after short time delays and more slowly after longer delays (see Appendix B).

As shown in Figure 3, application of the power decay model shows a precipitous decay in subjects' recalls of the issue gists for both candidates, with most of the forgetting occurring within one week after exposure to the campaign fact sheet. For example, considering only those subjects who were randomly assigned to the 50+ questions in the depth-of-processing manipulation (and thus have slightly better recollections), the power decay model predicts that within two days after exposure 25% of these subjects would be unable to recall any gists and after six days, the number of recalls would have dropped to zero for half of the subjects (see Appendix B). For the complex statements, recall declined even more steeply, with most of the forgetting occurring within one day after exposure. Moreover, when the recalls of all subjects were considered, still steeper forgetting curves were obtained (results not reported). The conclusion to be drawn from these analyses is clear: *memory for campaign messages not only fades but fades quickly*, the half-life of the message typically being less than a week.

The evidence so far clearly casts doubt on the viability of memory-based assumptions in models of electoral behavior. An obvious question, however, is

FIGURE 3

Forgetting Curves (Democrat)



Note: P(recall) is the probability of retrieving at least one item from memory (see Appendix B)

whether an on-line processing assumption fares any better. To shed light on this issue we would have to look (albeit indirectly) at the decay of the OL tally in memory—"indirectly" because the OL tally is a latent variable and, as such, unobservable. It is possible, however, to gauge memory for candidate evaluations, which may serve as proxies for the OL tally.

In determining memory for candidate evaluations we make the assumption that, with some degree of error, changes in candidate evaluation reflect an imperfect memory for the OL tally.[9] One possibility is to claim that subjects have forgotten the OL tally if they were capable of providing a candidate evaluation at t_1 (the time of exposure) but not at t_2. A second option is to attribute any change in candidate evaluation to decay of the OL tally. While the first criterion probably underestimates the amount of decay in the OL tally, the second criterion probably overestimates it because opinion change may take place for reasons other than forgetting.[10] The two criteria, then, provide a lower and upper bound on the decay of the OL tally. When we use these two bounds, the top two forgetting curves in Figure 3 result. These curves tell a very clear story: a majority of the subjects who provided both t_1 and t_2 candidate evaluations did indeed recall which of the two candidates they liked or disliked. Thus, while memory for the campaign issues declined steadily to low levels, memory for the overall evaluation of the candidates was remarkably robust and stable over time.

In sum, these analyses demonstrate that memory for campaign messages is weak: citizens forget a lot of campaign information rather quickly. By all normative standards—*were we to rely on recall*, the citizen would appear to be rather unaware of what goes on in political campaigns. At best, he or she may remember a thing or two about the candidates and their issue stands but it is clear (from the forgetting curves) that by the time a vote has to be cast much of the information that was attended to has faded from memory.

New Evidence on the Democratic Citizen: Voter Responsiveness

The evidence we have presented so far in essence replicates and expands received wisdom about the American voter. Voters, it would appear, lack the ability to recall essential campaign facts, even (as we have shown here) after short periods of time. And in keeping with received wisdom, one could easily infer from our evidence that voters, unable to recall the most basic issue positions of the candidates, are not politically sophisticated and (so the logic goes) that campaigns are inconsequential because little if any campaign information sticks in voters' minds. However, we counsel caution in drawing such a negative conclusion about the democratic citizen, for we hope to show that it is not the citizen who is at fault but, rather, the memory-based assumption.

We begin our reconsideration of the qualities of the democratic citizen by challenging the implicit assumption in memory-based models that recall is a critical mediator between campaign stimuli and candidate evaluations (see Price and Zaller 1993). We contend that campaigns can have an effect that is quite independent from recall. Instead of being mediated by citizens' recollections of the campaign specifics, this effect is mediated by the OL tally, which integrates the affective evaluation of all the campaign information the citizen is attentive to.

Although we do not have a direct measure of the OL tally itself, we do know what campaign information our subjects were exposed to and their prior evaluations of this information. We can thus correlate subjects' candidate evaluations with their affective responses to the issue positions that the candidates expressed in the campaign fact sheet, which we now label the *candidate message*. To the extent that candidate evaluations are strongly related to candidate messages, we would say that our subjects are responsive to the information in the campaign, *regardless of whether they can actually recall the campaign contents*. On the other hand, should it be shown that message exerts a substantively small effect on candidate evaluation, we would have to conclude that voters are unresponsive to the campaign and would join the ranks of those who question the quality of electoral decision making of democratic citizens. In the ensuing analyses we will move from simple through more rigorous tests of alternative on-line and memory-based models of the responsive voter.

The Role of Message in Candidate Evaluations. A first impression of the impact of message on candidate evaluations can be obtained by considering the zero-order correlations between these variables. Table 1 presents these correlations, with message operationalized as the average of a subject's evaluation of *all* the policy stands (gists and specifiers) taken by each candidate in the fact sheet. The message-evaluation correlations, while not overwhelmingly strong, are significant and in the right direction.[11]

TABLE 1

Message, Recall, and Candidate Evaluation (Pearson Zero-Order Correlations)

RELATIONSHIP	ALL SUBJECTS	DEPTH-OF-PROCESSING MANIPULATION ABSENT	DEPTH-OF-PROCESSING MANIPULATION PRESENT
Democrat	(n = 158)	(n = 84)	(n = 74)
Message-evaluation	.2846***	.1891*	.3700***
Recall-evaluation	.2377***	.1691	.2620**
Republican	(n = 160)	(n = 85)	(n = 97)
Message-evaluation	.3796***	.3623***	.3958***
Recall-evaluation	.1932**	.1391	.2622**

* $p < .10$.
** $p < .05$.
*** $p \leq .01$.

If the size of the message-evaluation correlations seems unimpressive, we should keep in mind that the recall-evaluation correlations are weaker still. Recall was operationalized here in a manner similar to message, namely as the average of the evaluations of all gists and specifiers that a subject retrieved from memory for a candidate.[12] If subjects could not retrieve any issue for a candidate the recall score was set to the neutral point.[13] Doing so, we find a rather weak recall-evaluation correlation for the Republican and a somewhat stronger correlation for the Democrat. In both cases, however, the correlation falls short of that observed between message and evaluation.

One circumstance under which one might expect the recall-evaluation correlation to exceed the correlation between message and evaluation would be when voters are stimulated to think hard about the campaign information, as did our subjects in the in-depth-processing condition. As Table 1 columns 2–3, indicate, the recall-evaluation correlation does indeed improve when in-depth processing is encouraged. However, even under these most favorable conditions for memory-based processing, the correlation between recall and candidate evaluation does not surpass the correlation between message and candidate evaluation. Thus, even under circumstances that are most congenial to the preservation of campaign information in voters' minds, candidate messages are more strongly correlated with the subjects' overall candidate evaluations than is the affective value of their recollections of campaign events.

Although the correlations are suggestive of the importance of message in the candidate evaluation process, stronger evidence can be obtained by considering recall and message simultaneously in a regression analysis. This analysis also includes party identification (PID) as a predictor of candidate evaluation, as partisanship captures important influences on the vote that are relatively independent of the campaign and its issues. Table 2 reports the regression results, with all variables scaled to a 0–1 range to make their regression coefficients directly comparable.

The results in Table 2 confirm the importance of message in the candidate evaluation process. Not only is message a significant predictor of candidate evaluation, but its impact is substantial, rivaling the effect of PID for the Democratic candidate. Because this candidate's policy stands were somewhat at odds with our subjects' stereotypical image of a Democrat, they added substantially to whatever information the partisan label revealed. Apparently, our subjects were responsive to the somewhat discrepant nature of the Democratic candidate and did not rely solely on their partisan feelings to guide their candidate evaluation.

Another striking result of the regression analyses in Table 2 is that recall does not play much of a role at all. Once we control for the impact of message, recall either becomes statistically insignificant or has a much diminished effect on candidate evaluation.[14] The importance of message for candidate evaluations is illustrated by the fact that the fit of the model deteriorates significantly when message is eliminated as a predictor. Comparisons of the three-variable regression analyses reported in Table 2 and the more common regression of recall and PID on evaluation show how critical message is: F[1, 141] = 8.674, $p < .01$, for the Democratic candidate; and F[1, 143] = 8.177, $p < .01$, for the Republican. In contrast, eliminating recall from the regressions in Table 2 does not greatly affect the model fit: F[1, 141] = 3.287, $p < .10$, for the Democrat; and F[1, 143] = .608, *ns*, for the Republican.

Two important conclusions may be drawn from the regression results. First, in contrast to what political scientists often assume (Kelley 1983; Price and Zaller 1993), recall is not a necessary condition for political

TABLE 2

Message, Recall, PID, and Candidate Evaluation (OLS Regressions)

PREDICTOR	CANDIDATE EVALUATION DEMOCRAT *B*	CANDIDATE EVALUATION REPUBLICAN *B*
Message	.384*** (.130)	.299*** (.105)
Recall	.149* (.082)	.055 (.069)
PID	−.330*** (.053)	.407*** (.057)
Intercept	.472*** (.093)	.131** (.059)
Adjusted R^2	.291	.378
Standard Error of Regression	.190	.190
n	145	147

Note: All variables are scaled in the 0–1 range. Estimated standard errors appear in parentheses.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

information to be influential on the judgments and decisions that democratic citizens make. On the contrary, once the impact of message is considered the effect of recall is greatly reduced. We emphasize this point because it helps account for why experiments and surveys produce different results about the impact of recall on evaluation. Surveys cannot control for message; consequently, much of its effect is absorbed by recall. By taking message explicitly into account, as we did in our experiment, recall plays a lesser role. Thus, the strong showing of recall in surveys may well be a statistical artifact, that is, a product of researchers not specifying the logically prior effect of message on the judgmental process.

Of greater importance than the methodological implication of considering message are the substantive implications for democratic theory. In contrast to the thrust of much empirical work in electoral behavior, we trumpet V. O. Key's dictum: "Voters are not fools." From the on-line perspective, voters are seen as actively bringing their evaluation of the campaign issues to bear on their political evaluations and choices, this being especially apparent when a candidate deviates a bit from partisan expectations. In our view, the regression analyses in Table 2 can be readily interpreted in a straightforward way: voters are responsive to political campaigns. No matter how little voters may recall from the campaign, our evidence suggests that *if* attentive to campaign issues and events, they use this information to inform their candidate evaluations.

Caveats. Although we view our results as convincing evidence for voter responsiveness, several questions might be raised. First, it could be argued that the limited effect of recall is a methodological artifact of how we coded missing data to the neutral midpoint of the evaluation scale. By assigning the numerous subjects without retrievals to a neutral point we may have greatly reduced the variance of our recall measures for both candidates (see Little and Rubin 1987), thereby stacking the deck against finding significant recall effects. This does not prove to be a problem, as the variances of the recall measures exceeded those of message. Nonetheless, we reestimated the candidate evaluation models for the subgroup of subjects who had at least one retrieval for a given candidate. Doing so hardly affects the results reported in Table 2. Indeed, the results (unreported) for the Republican candidate are almost unchanged. The results for the Democrat show a weakening of the effect of message but *not* to the advantage of recall. In fact, the recall effect becomes somewhat weaker when we consider only those subjects who could recollect at least one pro or con for the Democrat, and it is overshadowed by the effect of message. We consider this evidence that our results concerning the limited role of recall are not a statistical artifact.

A second concern that could be raised is that our findings are an artifact of the way we constructed message and recall. This is a substantively more interesting issue, because it directly pertains to the way we believe information is integrated. So far, we have postulated a simple additive rule, whereby the evaluations of all information items are equally weighted and then summed. However, this is only one of many possible information integration rules, albeit one that is rather prevalent in political science. Hence it is legitimate to ask whether our results would look different if we were to use a different integration rule.

Although this is not the place to discuss the many ways in which voters can combine campaign information (see Taber and Steenbergen 1994), we did replicate our analyses employing an information integration rule that is most favorable toward recall, namely Kelley and Mirer's (1974) "simple act of voting," which (as noted earlier) is a straightforward memory-based model. This rule postulates that voters classify the issues that they recall into likes and dislikes. For each candidate, the voter subtracts the dislikes from the likes to arrive at net liking scores. The candidate with the largest number of net likes is the preferred candidate, with PID acting as a tie-breaker in the case where two candidates receive the same net liking score.

Table 3 reports the results for the Kelley–Mirer information integration rule, as applied first to recall and then (by extension) to message, operationalizing this as the between-candidate difference in net liking scores for all the statements in the candidate fact sheet. Since recall and message are on different scales we report both unstandardized and standardized regression coefficients. The dependent variable in the analysis is the difference in evaluation between the Democratic and Republican candidates.

The results for a recall-only model, which are reported in the first two columns, are congruent with what Kelley and Mirer (1974) find, namely, significant recall effects and critical effects from PID as a tie-breaker.[15] In addition, there is a strong effect from recall tie. If recall and PID are set to 0, so that the recall tie dummy is 1, our subjects favored the Democratic candidate (intercept is 2.086 + .443). If recall is not 0, so that there is no recall tie, the baseline evaluation of the candidates was about neutral (intercept is .443).

The role of recall dwindles, however, when we include the difference in net liking scores for candidate messages. As Table 3, columns 3–4 demonstrate, the results for the Kelley–Mirer information integration rule mimic the results in Table 2, showing a significant and substantively sizable effect from message and an insignificant effect from recall ($p < .15$). Note also that the standardized regression coefficient for message is considerably higher than for recall. This finding not only illustrates the robustness of the message effect across different information integration rules but also offers an interesting commentary on Kelley and Mirer's (1974) model. The strong memory-based effects that their model suggests do not appear to be very robust: as soon as message is entered as a control variable, the importance of recall as a determinant of candidate evaluation fades.

TABLE 3

Message, Recall, PID and Candidate Evaluation: Kelley–Mirer Model (OLS Regressions)

	RECALL ONLY		RECALL AND MESSAGE	
PREDICTORS	B	β	B	β
Message [Net Likes(Dem) − Net Likes(Rep)]	—	—	.076*** (.017)	.339
Recall [Net Likes(Dem) − Net Likes(Rep)]	.296*** (.079)	.283	.125 (.083)	.120
Recall Tie	2.086*** (.482)	.643	1.420*** (.484)	.438
Message Tie	—	—	1.380 (2.395)	.119
PID * Recall Tie	−.806*** (.137)	−.857	−.616*** (.138)	−.655
PID * Message Tie	—	—	−.503 (.620)	−.168
Intercept	.443** (.179)	—	.082 (.191)	—
Adjusted R^2	.269		.348	
Standard Error of Regression	1.385		1.308	
n	152		152	

Note: Dependent variable is difference in candidate evaluations between the Democrat and Republican. Estimated standard errors appear in parentheses.
** $p < .05$.
*** $p < .01$.

While the critical role of message in candidate evaluation appears to be robust, skeptics might question our assumption that the voters are responsive to the entire message rather than parts of it. Specifically, the question is whether voters only use their gist evaluations to judge political candidates or whether they also bring the candidates' elaborations and qualifications into play. The answer to this question is important for determining exactly how responsive voters are to political campaigns.

We assessed the extent of voter responsiveness through a model comparison test. In a first model we predicted the evaluation of each candidate by the average evaluation of that candidate's complex statement (both gists and specifiers). The second model eliminated the specifiers, that is, the average evaluation of all specifiers to an issue gist, allowing us to compare the fit of this gist-only model with the full model. In both models, PID was entered as a control variable. Eliminating the specifiers significantly decreases the model fit significantly: F[7, 110] = 2.113, $p < .05$, for the Democrat; and F[7, 123] = 1.855, $p < .10$, for the Republican. It appears, then, that our subjects took the entire message into account in their candidate evaluations and not just the general themes.

The Candidate Evaluation Process: A Causal Model

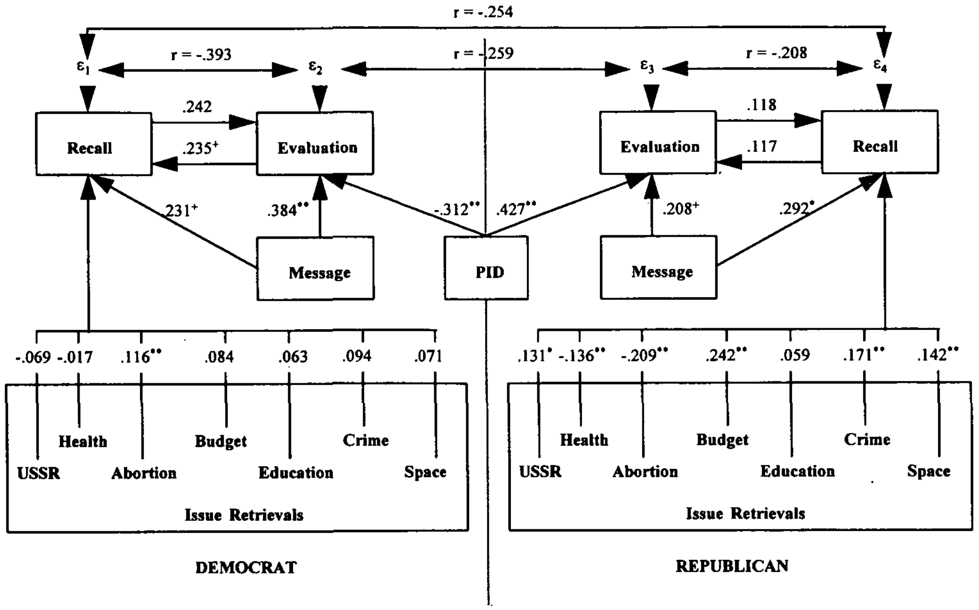
The results up to this point provide strong evidence for our theoretical claims. First, voters do seem to incorporate campaign information in their candidate evaluations; that is, they are responsive. Second, recollections of the campaign play at best a modest role in the candidate evaluation process and are clearly subsidiary to the impact of message. So far, however, our analyses have fallen short of considering the model proposed in Figure 1. That is, we have so far given recall and message the same causal status, although we know that message is logically prior to recall. Moreover, we have so far assumed that recall causes candidate evaluation, while our theory suggests that voters' recollections may actually be the product of these evaluations. This implicit acceptance of a basic premise of memory-based candidate evaluation models may have resulted in the overestimation of recall effects.

Figure 4 displays a model that more closely corresponds to the theoretical notions set forth in Figure 1, adding the distinctive feature that it considers both candidates simultaneously. The candidate evaluation portion of this model is similar to the regression analysis in Table 2: candidate evaluation is modeled as a function of recall, message, and PID.

The recall portion of our model reflects three possible mechanisms that can generate the affective value of voter recollections. First, recall can be driven by whatever issues a voter happens to retrieve from memory for a given candidate. This mechanism does not assume a particular evaluative direction in the retrieval of issues. No assumption is made, for example, that retrievals are more likely for issues that were evaluated consistently with subjects' candidate evaluations. Indeed, the correlation between the issue retrieval dummies (0 = no retrieval, 1 = retrieval) and

FIGURE 4

Causal Candidate Evaluation Model



Note: n = 144. Model fit: Normal theory $\chi^2 = 57.847$, df = 46, $p = .113$; Robust $\chi^2 = 62.469$, df = 46, $p = .053$; Comparative Fit Index (CFI) = .998. All variables scaled in 0–1 range. ** $p < .01$, * $p < .05$, † $p < .10$.

candidate evaluations tends to be low and is, except for one out of 14 cases, statistically insignificant. (The average polychoric correlation between candidate evaluation and issue retrieval is .143 for the Democrat and −.090 for the Republican).

The two other mechanisms that may drive recall are directional in nature. One of these mechanisms postulates that recall is a function of candidate message. Specifically, recall is assumed to absorb the evaluative implications of the campaign, such that retrievals are a fair sample of the campaign items to which voters were exposed. Alternatively, we postulate a rationalization effect whereby recall is driven by candidate evaluation. If existent, we expect this effect to be positive, meaning that retrieval is biased in favor of items that are affectively consistent with one's overall evaluation of the candidate.[16] Notice that this biased retrieval effect competes with the first recall effect that we described.

Three further comments are in order for the model depicted in Figure 4. First, no effect from PID on recall was specified, since both variables are hardly correlated ($r = -.061$, *ns*, for the Democrat; $r = .119$, $p < .10$, for the Republican). Second, the model contains correlated errors ($\epsilon_1, \ldots, \epsilon_4$) between the recalls and evaluations of each candidate, between the recalls for both candidates, and between the evaluations of both candidates. Finally, all variables in Figure 4 are scaled in the 0–1 range, so that unstandardized parameter estimates are directly comparable.[17]

The model in Figure 4 fits the data adequately (as indicated, in particular, by the Comparative Fit Index) and reinforces the main findings of our previous analyses. That is, we find a sizable and statistically significant effect of message on candidate evaluation, while the effect of recall is substantively small and not significant. Figure 4, however, also shows that it can be quite misleading to assume that recall is causally prior to candidate evaluation. Although only marginally significant, Democratic candidate evaluation has a sizable and positive effect on the recall for this candidate, indicating the presence of a biased retrieval mechanism. Note that this effect is similar in magnitude to the effect of message on recall, suggesting that retrieval may be as much the product of voters' judgments of a candidate as it is the product of their processing of campaign messages. As a consequence, recall may be a very poor proxy for measuring how voters respond to campaign information.

It should be noted that we only find an effect of candidate evaluation on recall for the Democratic candidate. Note that unlike the Republican candidate, who espoused a consistent party line, the Democrat "trespassed" on Republican turf on two of his seven issue positions. Consequently, this candidate was not only liked by subjects who identified themselves as Democrats but also by about 36% of the Republican identifiers, who favored these Republican policies (sometimes more so than the Democrats). The latter group, in particular, may have felt a need to rationalize their positive evaluations of a Democrat, hence recalling issues that were consistent with their candidate evaluation. No such similar need existed for Democratic identifiers with respect to the Republican candidate, since only 10% of the Democrats liked this candidate. This may account for the insignificance of biased recall for the Republican and the stronger showing of the first recall mechanism that we postulated, namely, recall based on whatever issues voters retrieve from memory. Of course, it only reinforces our main conclusion if our subjects were so sensitive to the stereotypicality of candidates: voters are responsive to the features of a campaign.

CONCLUSIONS

Our results point in two directions. First, voters appear to be responsive to the campaign information that they receive, even in the low-stimulus election that we presented to our subjects. Second, this responsiveness is not reflected in voter recollections of the campaign. Even after a short delay voters are unable to recall many of the campaign facts and this includes circumstances in which the number of facts is small and the conditions for memory-based processing favorable. These results have important methodological, substantive, and normative consequences.

Methodological Implications. Methodologically, our findings suggest that it is easy to overstate the role of memory in political judgment when all that is available are survey data. Models of the vote choice typically find strong effects of recall on candidate evaluations in survey data. Surveys, however, can neither measure directly nor control for what information voters are actually exposed to during the campaign. Consequently, they are unable to determine the direct impact of campaign events on political judgment and choice.

Our findings suggest that proper control for campaign exposure will dramatically reduce the effect from recall to the point that it is unclear that the recollection of campaign events matters much at all. The required controls, however, can practically only be obtained in an experimental setting where the researcher has the ability to systematically influence the amount, type, and timing of the information that subjects receive. Hence, we believe that statistical inferences about the role of recall in political judgment are better made with experimental than with survey data.

Substantive Implications. Substantively, our results provide commentary on three topics central to the study of electoral behavior: (1) our understanding of the bases of voting behavior, (2) our understanding of campaigns, and (3) our understanding of political information processing.

In terms of the bases of electoral behavior, our results give further credence to earlier criticisms of memory-based models of candidate evaluation (Lodge, McGraw, and Stroh 1989; Lodge and Stroh 1993). Here, in looking at recall over more realistic time intervals and under circumstances ideally suited to memory-based information processing, recall played at best a modest role in the candidate evaluation process, after message had been controlled for. We view this as evidence that we should integrate message-based models of vote choice into the analysis of electoral behavior.

An emphasis on the content and structure of candidate messages (see Rahn, Aldrich, and Borgida 1994) might well lead to a more positive appreciation of the role of campaigns and media effects. If recall were the only mechanism through which campaign information could affect political judgment and choice, then our data suggest that campaigns are inconsequential for most voters. However, we find that campaign messages have a substantial impact on evaluations independent of recall. The way in which this influence was brought about in our experiment mimics Lazarsfeld, Berelson, and Gaudet's (1948) understanding of campaign effects. That is, we measured prior attitudes about a range of issues and assumed that these attitudes would be mobilized by the campaign as candidates addressed the issues. Our results indicate that this type of campaign effect can be very strong, adding sustaining evidence to prior work on the importance of campaigns (Bartels 1993; Marcus and MacKuen 1993).

Finally, our results suggest a very different model of information processing than is oftentimes assumed in political science. Our discipline relies too heavily on—and often draws the wrong conclusions from—the assumption that political information can only exert an effect on political judgment if citizens remember it. To cite Price and Zaller, "For many—and quite possibly most—survey research purposes, news stories that are encountered but not comprehended and *retained* have little importance because they have few detectable consequences for the person's store of information, attitudes, opinions, or behaviors" (1993, 135; emphasis ours).

In contrast to Price and Zaller (and many other scholars) we believe there are many circumstances (political campaigns being a case in point) under which memory does not play a critical mediating role. We have demonstrated that campaign facts can exert an influence over political judgment even for subjects who could recall few if any of these facts. The mechanism that we propose for these campaign effects is a version of the on-line model of information processing: the campaign raises issues that mobilize issue opinions that voters subsequently integrate, along with other factors like PID, into a running affective tally for each candidate. In this process recollections do not play a decisive role, short of the requisite that the OL tally be recalled.

Normative Implications. In our view, the most important conclusion to be drawn from this research lies in the normative domain. Much has been said about the failure of citizens to live up to democratic standards (see Hanson and Marcus 1993). Often these standards have been stated in terms of how much political information citizens can recollect in reply to knowledge questions or open-ended questions about their likes and dislikes of candidates, parties, and issues. Our findings run counter to this outlook: we suggest that information holding is but one standard of good citizenship and not necessarily an appropriate or important one (see Graber 1984). Rather than judge the citizen as we would a student taking an exam, we think it far more valid to judge the citizenry in terms of how much impact this information has on their judgments. What is important is not so much whether citizens can recall a little or a lot of information but that their political judgments and choices reflect their evaluation of the information. Because recall is, as we have seen, not a critical mediator of campaign information, the question about the impact of information is quite independent from the question of recall.

This being the case, some recasting of research questions is called for. If the issue is not how much information people can recollect about the candidates but how much of what kinds of information they actually entertain over the course of the campaign, then we need to focus on what campaign information is available and the conditions under which citizens will make the cognitive effort to mull over this information and expend scarce attentional resources to integrate the affective value of messages into an overall impression of candidates and issues. From this bounded rationalist perspective, a critical question for democratic theory focuses on the information integration rules people use to increase or decrease their OL tallies. Only when this question is answered can we then ask whether responsive voters are also reasonable people.

APPENDIX A

In stage 1c, subjects read the campaign fact sheet presented in Table A-1.

TABLE A-1

Candidate Fact Sheet

	DAVE WAGNER, REPUBLICAN	TOM MESSINGER, DEMOCRAT
Background	Born: February 11, 1944. Hometown: Erie, Pennsylvania. Education: A.B., University of Pennsylvania; LL.B., Washington University. Career Highlights: U.S. Navy (1963–1967), City Councilman (1972–1974), Assistant Director of State Budgeting (1982–1986), and State Senator (1986–present). Married to Ruth, two children—Betty and Mary.	Born: March 4, 1943. Hometown: Greensburg, Pennsylvania. Education: B.A., M.B.A. Slippery Rock School of Business. Career Highlights: U.S. Air Force (1962–1965), County Commissioner (1968–1974), State Assemblyman (1976–1984) and State Secretary of Commerce (1986–present). Married to Gladys, three children—Frank, Beth and Peter.
Soviet Union	America should provide technical assistance to the newly independent Soviet republics in order to improve the internal distribution of food and establish an economy based on private-ownership. Dave also favors granting most-favored-nation trading status to the newly formed governments.	The United States ought to help the Soviet Union continue its democratic and economic reforms. This includes food shipments, low-interest loan guarantees, and diplomatic recognition of the independent republics.
Health care	Dave believes the government is not the solution to the nation's health care crisis. He opposes programs that force businesses to provide health care to their workers. Instead, Dave advocates limits on damage awards in medical lawsuits, more competition among medical professionals, and responsible health habits.	Tom supports the establishment of a federal health insurance program by requiring businesses to insure their workers or contribute to a government health-insurance fund. The program would include prenatal and infant check-ups, screening for common cancers, and regular physical examinations.
Abortion	Dave Wagner wants to prohibit abortions in all cases, except those threatening the mother's life. He also advocates the counseling of pregnant women about adoption, education programs that emphasize sexual responsibility and abstinence, and over-turning the Supreme Court's *Roe* v. *Wade* decision.	Tom supports the abortion rights of women. He also supports the prevention of unwanted pregnancies. counseling pregnant women on their medical options, including abortion, and rules allowing Medicaid to pay for abortions for poor women who are victims of rape and incest.
Budget deficit	Raising taxes is an anti-growth policy, and gives our economic competitors an unfair advantage. Instead, the government should cut unnecessary programs and improve efficiency in order to comply with the Deficit Reduction Act.	To meet the deficit-reduction targets, the government should improve its efficiency and eliminate non-essential programs. Under no circumstances should the government raise taxes to balance the budget.

(continued on next page)

TABLE A-1 (continued)

Candidate Fact Sheet

	DAVE WAGNER, REPUBLICAN	TOM MESSINGER, DEMOCRAT
Education	Dave supports the reform of our nation's education system. He advocates merit-based salary increases for teachers, nation-wide standards in basic school subjects, and school voucher-systems, which give parents the right to choose the best school for their children.	Tom favors an expansion of federal education programs. This includes the establishment of nation-wide testing for high-school student competency, giving parents the right to choose the best school for their children, and the creation of federally financed "magnet-schools."
Crime and drugs	Dave favors an aggressive war against crime and drugs in our neighborhoods. This includes the interdiction of drugs before they cross our borders and police crack-downs on casual drug-use. Furthermore, he supports the confiscation of drug-offender possessions and the death penalty for drug-related murder.	Tom supports federal laws which crack-down on drugs and crime. He favors the death penalty for murderers, mandatory sentences for drug king-pins, and a mandatory waiting period and back-ground check to purchase a firearm. Tom also supports treatment programs for drug addicts.
Space program	The United States should expand NASA's space program. This includes the construction of new space shuttles and the eventual establishment of a permanent base on the Moon.	Tom favors the re-ordering of our priorities in outer space. He wants to cut the space station budget, and use unmanned rockets to lift satellites into orbit.

APPENDIX B: THE STATISTICAL ANALYSIS OF MEMORY DECAY

Cognitive and experimental psychology have had a long tradition of describing the functional form of forgetting over time. Researchers have adhered traditionally to Ebbinghaus' (1964) seminal work on forgetting curves, which relied on logarithmic functions. However, recent evidence, based on a wide variety of experimental conditions and response modalities, suggests that a power decay function provides a better and more general description of memory decay (Wixted and Ebbesen 1991; see also Wickelgren 1974). This decay function is commonly formulated as $y = \alpha t^{\beta} + \epsilon$ over the domain $t \in <0, \infty>$. Here y is a recall performance measure (e.g., the number of items from a list that can be recalled), ϵ is a disturbance term, t is the delay between exposure to information and its recall, and $\beta \leq 0$ is a decay parameter that determines the decay rate $[dy/dt = \beta(y/t)]$. Given the form of the decay rate it is clear that the power decay model implies that memory decay slows down as the delay increases or as the amount of information in memory decreases.

The final parameter of the power decay model is $\alpha \geq 0$. This is a scaling parameter that ensures that y is not automatically 1 when t equals 1. This would be undesirable, because y can be measured on many different scales, depending on what recall performance measure is used and how much information subjects were exposed to. A substantive interpretation of α is that it gives the recall performance after a time delay of one unit (e.g., a day).

The power decay model is usually applied to recall performance measures that assess how much information is retained in memory. Information-based performance, however, may be problematic to analyze statistically when only one information item was attended to. In that case y is a dichotomous variable (subjects either recalled or did not recall the information item), and all the standard problems with analyzing dichotomous variables (e.g., heteroscedasticity) will enter the analysis. Solutions for these problems are readily available for linear models that are estimated through ordinary least squares, but equivalent solutions for nonlinear models are less straightforward.

To overcome the statistical problems involved in analyzing the power decay model for dichotomous recall-performance measures, we constructed an alternative measure that is both intuitive and easy to handle in statistical analysis. We define recall performance as the *proportion* of subjects who were exposed to some information set of one or more items and who could recollect one or at least one item after t units of delay. This definition permits the estimation of forgetting curves in cases where only one item is subject to memory decay, as in the case of candidate evaluations.

Given the generality of the power decay model, we believe that a definition of y in terms of proportions of subjects is appropriate. This definition causes no special problems statistically. The power decay model for proportions can be estimated through standard nonlinear regression algorithms, subject to a specific loss function. We employed a sequential quadratic programming algorithm that minimizes the sum of squared residuals (Thisted 1988). Estimation was subject to the normal constraints—$\alpha \geq 0$, $\beta \leq 0$—as well as the additional constraint, $\alpha \leq 1$, that was imposed to secure that the predicted values of y are in agreement with the probability axiom that proportions cannot exceed 1. The estimates were weighted to accommodate the fact that y is a statistic (i.e., a proportion) that may be based on different sample sizes for each t.

TABLE B-1

Memory Decay Models (Nonlinear Regressions)

MODEL (SUBJECTS WITH IN-DEPTH PROCESSING)	ESTIMATED α	ESTIMATED β
Recall Democratic candidate		
Evaluation (upper bound on memory decay)	.714*	−.015
Evaluation (lower bound on memory decay)	.811*	.000
Gists (≥1 gist)	.953*	−.382*
Specifiers (≥1 specifier)	.247	−.219
Recall Republican candidate		
Evaluation (upper bound on memory decay)	.730*	−.127
Evaluation (lower bound on memory decay)	.839*	−.016
Gists (≥1 gist)	.955*	−.369*
Specifiers (≥1 specifier)	.358	−.233

* $p < .05$ (based on bootstrapped standard errors).

Notes

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1. Whether the rules for integrating information into a summary judgment are normatively correct or not is a different question. See Taber and Steenbergen (1994) for a description and analysis of various decision rules for integrating information into vote choice.

2. The notion of voter responsiveness was introduced earlier by Pomper (1975) to distinguish voting behavior that is attentive to the issues of the day from behavior that is driven only by long-term or stable factors such as race and party identification. Our usage of the term is essentially in line with this definition, although our approach is quite different from that by Pomper.

3. When both issues and candidate characteristics were considered the percentage of recalls increased somewhat, although not by very much. In keeping with most voting models, we will consider only issue recalls in the remainder of this paper. However, had we included recalls of candidate characteristics as well, our results would have essentially been the same.

4. Memory-based models of candidate evaluation are generally oblivious about whether recalls are factually correct or not. For this reason we analyzed the recall data without regard for their validity.

5. Specifier recalls were only counted when a subject also recalled the corresponding gist, that is, when a subject recalled the complex issue position. This restriction was imposed because specifiers only make sense in the light of an associated gist.

6. The political knowledge scale consisted of six items concerning the party affiliation of national political figures. Each item was scaled from -1 (incorrect response) to 1 (correct response), with 0 indicating *don't know.* The items form a moderately strong stochastic cumulative scale under Mokken's scale model ($H = .54$), with a decent reliability of .754 (Sijtsma, Debets and Molenaar 1990). Although the political knowledge items are themselves memory-based, we are confident in using them as independent variables, because the memory task involved is rather distinct: the knowledge questions rely on recognition, whereas recollections of the campaign information were obtained through cued recall.

7. The combined recall for both candidates was one type of dependent variable that was analyzed in the repeated-measures ANCOVAS. The other dependent variable was the difference in recalls between the two candidates, which permits an analysis of the within-subjects manipulation of partisan consistency. Since no significant effect of this manipulation was found, we will only report the results for the combined candidate recalls.

8. Considering Average Importance of Message (Sum of Importances for All Issues ÷ Number of Issues) − Average Importance of Recalls (Sum of the Importances for All Recalls ÷ Number of Recalls), we obtained the following test results: $t(76) = 1.567$, $p = .94$ for the Republican; $t(66) = 1.006$, $p = .84$ for the Democrat.

9. This assumption is reasonable for two reasons. First, the campaign fact sheet was the only information that we disseminated to our subjects, so that changes in candidate evaluation cannot be attributed to the intake of new information about the candidates. Second, because the race between the candidates was low-stimulus (many of our subjects admitted to have little interest in the race) it is questionable that our subjects spent much time in rethinking the candidates' issue positions. Hence, cognitive responses to the information are an implausible mechanism for producing changes in attitudes toward the candidates.

10. One reason for opinion change, of course, is measurement error. To reduce this problem we assessed change in candidate evaluation in terms of collapsed versions of the candidate evaluation scales, distinguishing only between negative, neutral, and positive feelings toward a candidate. Opinion change was defined as the movement from one affective category to another.

11. *Candidate evaluation* always refers to the judgments of the candidates that subjects gave in the t_2 portion of our study.

12. Because we now define recall in evaluative terms, we will no longer use it to describe the act of retrieving information from memory. This is now referred to as *retrieval.*

13. This procedure implies that subjects without recalls are equated with subjects who recall items that they feel neutral about or subjects who recall a mix of items that neutralize each other. We view this as imminently reasonable, since a lack of recalls should have the same effect as neutral recalls, namely that voters cannot decide which candidate they like best on the basis of their issue recollections.

14. Analyses with recall and PID as the sole predictors of candidate evaluation show a significant recall effect for the Democratic candidate ($b = .221$, $p = .007$). A similar analysis for the Republican candidate reveals an insignificant effect of recall, but its effect size is almost twice as large as that in the analysis reported in Table 2.

15. In this analysis we adopted the methodologically unconventional method of specifying an interaction with PID without including PID itself in the regression. Although this specification preserves the tenets of the Kelley–Mirer information integration rule, we did reestimate the model including PID as a main effect. Doing so, we find that the interaction of PID and recall ties becomes statistically insignificant, while the main effect of PID is highly significant. However, with respect to the relative impact of message and recall, nothing substantial changes in the patterns reported in Table 3.

16. A positive effect of candidate evaluation on recall can imply one of two things. First, there may be biased retrieval. Second, voters may have unbiased retrieval but change the affect for retrieved items so as to be consistent with their candidate evaluation. We have precluded this latter mechanism by using the affective values for gists and specifiers that were measured before our subjects were exposed to the candidates.

17. The model was estimated through EQS version 4.0 using generalized least squares. Because of a lack of normality of the endogenous variables conditional on the exogenous variables normal standard errors and test statistics are incorrect. Therefore, robust standard errors were obtained, while the Bentler–Satorra scaling correction was applied to obtain a correct model test (see Bentler 1992).

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THE ELECTORAL CYCLE AND INSTITUTIONAL SOURCES OF DIVIDED PRESIDENTIAL GOVERNMENT

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Presidents often lack legislative majorities, but situations of opposition-party majorities ("divided government") are much less common outside the United States. The president's party's share of seats tends to increase in early-term elections but decline in later elections. Thus opposition majorities often result after midterm elections. Opposition majorities rarely occur in elections held concurrently with the presidential election but are more likely to do so if legislators enjoy electoral independence from their parties due to features of electoral laws.

Divided government has been a common occurrence in the United States. A substantial literature has developed,[1] mostly arguing that divided government leads to undesirable policies and interbranch stalemates (e.g., Cutler 1980; McCubbins 1991; Robinson 1985; Sundquist 1986), although at least one important work (Mayhew 1991) has challenged the negative assessment. Surprisingly, there seems to be no literature specifically devoted to divided government in other presidential systems. However, almost all of the more general literature on presidentialism (e.g., Linz 1990, 1994; Mainwaring 1993; Valenzuela 1994) argues that lack of presidential majorities imperils government stability.

In a two-party system like that of the United States, a president lacking a copartisan majority is the same as a president facing an opposition majority. However, in the multiparty systems typical of most other presidential systems, these phenomena must be kept conceptually distinct. I shall use the term *divided government* to refer only to those situations in which a legislative majority is held by a party or preelection coalition that is different from that of the president. Divided government of this sort can be seen as a form of coalition government (Sundquist 1988)—indeed, as a grand coalition leaving no room for a distinction between government and opposition. The result is often alleged to be a lack of accountability, as no party is uniquely responsible for policy and there is no "out" party to which voters can turn if they want a change.

A situation in which no party holds a majority in a presidential system likewise can be seen as a form of coalition government. However, unlike opposition-majority situations, no-majority situations do not necessarily mean the absence of an opposition that can serve as an alternative governing formula. That is, although the category of no-majority situation is a residual category (neither "unified" nor "divided"), it is useful to keep it distinct, as it includes phases in which the chief executive's party, albeit a minority in the legislature, may be a part of most legislative coalitions along with one or more other parties. Such situations in parliamentary regimes do not necessarily obviate accountability or majority rule (Powell 1989; Strom 1990), as opposition-majority situations (divided government) may.

As we shall see, no-majority situations are much more common than divided government in presidential systems outside the United States. They are about as common as unified government. Thus it may be surprising to see the literature on presidential systems outside the United States speaking of such themes as "dual democratic legitimacies" (Linz 1994), conjuring up images of an executive and congress at loggerheads because each responds to a different majority of voters. The problem of competing legitimacies would seem to be most acute under divided government, yet one never encounters the phrase *dual legitimacies* in studies of the United States. Indeed, these two literatures on presidentialism (studies of divided government in the United States and studies of other presidential systems, esp. in Latin America) rarely address one another. It is time to start putting these literatures together to seek a more comprehensive explanation of the phenomenon of nonunified government. Then we may be in a better position to explain why divided government in the United States apparently does not cause such bad performance as "mere" nonmajority government allegedly does elsewhere.

I shall not attempt to resolve the debate about why presidentialism survives in the United States but often fails elsewhere. Nor shall I attempt to discern whether divided government produces better or worse governance than unified party control, though my conclusions have implications for these questions. Rather, I shall seek to answer a prior question, Why do patterns of divided, unified, and no-majority government vary? Do basic institutional differences among presidential systems give us much leverage over this question?

Party system variables are obviously important, but a reliance on such factors alone would not tell us if there are features in the design of constitutional and electoral rules themselves that affect the probability that the president's party will hold a majority or face majority opposition. For instance, divided government is likely to be more common in two-party systems than in multiparty systems, but so is unified government, for the simple reason that one of two parties must have a majority. While the occurrence of unified or divided government is thus mediated through the party system, there may be other structural variables that affect the pattern of presidential or opposition majorities by either (1) making it more likely that voters will vote for different parties for each of the two branches, or (2) reducing the number of parties.

One feature of institutional design that I find to be especially important is the timing of elections, or (as I shall call it here) the *electoral cycle*. In the data analysis that follows, I find that opposition majorities are uncommon when elections are concurrent, that is, when presidential and legislative elections are held on the same day. Expanding on earlier studies of the timing of elections (Jones 1994; Shugart and Carey 1992), I find that as elections are held later in a president's term, the share of seats won by the president's party tends to decline. Therefore, later-term elections often result in opposition majorities. This is a generalization of the well-known phenomenon of "midterm decline" in the United States (Erickson 1988; Hinckley 1967; Kernell 1977; Tufte 1975).

The second factor is the "localizing" or "nationalizing" effect of legislative electoral rules. Localizing rules are those in which candidates and incumbents can tailor their campaigns to suit local constituencies rather than rely mainly on voters' perceptions of the national party to which they belong. I argue that in such systems, as in the United States, there is a lesser sensitivity of legislative elections to the presidential electoral cycle. Thus occasionally divided government results even in concurrent elections. I show, too, that localizing electoral rules reduce the number of parties if elections are concurrent. Other things equal, a lower number of parties would mean a greater frequency of majorities for either the president's party or for the major opposition party. A third factor, the presidential veto—which may encourage voters to split their tickets if they desire more moderate policies than either of two major parties could deliver through unified government (Ingberman and Villani 1993)—does not appear to have a significant effect on the probability of an opposition majority.

Below I review some of the causes of divided government given in the U.S. literature and develop a theoretical justification for why the electoral cycle and electoral rules might matter for the observed variations in the tendency towards divided government. I present data on the incidence of presidential and opposition majorities and analyze the effects of the electoral cycle in more detail and explore how the incidence of divided government is mediated through the effects of institutional variables on the party system. Finally, I test for any effect of presidential vetoes on the probability of opposition majorities.

CAUSES OF DIVIDED GOVERNMENT

The Electoral Cycle and "Midterm Decline"

The electoral cycle refers to the relative timing of elections. In explaining divided government in the U.S. case it would have been sufficient, at one time, to look to this. Before 1956, divided government was almost exclusively a result of midterm elections. No concurrent election in the twentieth century and only two in the previous century (1876 and 1884) had produced a majority in either chamber for a party other than that of the incoming president.[2] Over the whole period from 1868 to 1992, opposition majorities have resulted from 21.9% of concurrent elections. On the other hand, the frequency of divided government resulting from midterm elections was more than twice as high—48.4%. Clearly the electoral cycle is only part of the explanation for divided government in the United States, but it is a big factor.

The literature on the United States is necessarily restricted to the dichotomous classification of congressional elections into *concurrent* and *nonconcurrent* (usually called *on-year* and either *off-year* or *midterm*). Still, we can turn to the U.S. literature for guidance as to why the electoral cycle might matter. To the extent that the timing of an assembly election influences the degree of convergence or divergence of votes for the two branches, it is also likely to have an effect on the incidence of divided government. I shall not pretend to review comprehensively the immense literature on the phenomenon of midterm decline in the United States. However, we need to consider some of the main strands of theorizing on the topic.

The most obvious answer to the question why there are midterm losses is simple: over time, more voters become disillusioned and defect to the opposition (see Kernell 1977). However, there are many explanations that either compete with or embellish this one. Some scholars have stressed differences in the composition of the electorate in concurrent and midterm elections (Born 1990; Campbell 1960). Others have seen midterms as referenda on presidential popularity or economic performance (Kernell 1977; Tufte 1975) or as a means of moderating policy by strengthening the opposition (Alesina and Rosenthal 1989). Some see midterm losses driven mainly by larger-than-average gains in the preceding concurrent election (Hinckley 1967). Erickson (1988) considered these and other explanations and concluded that a simple notion of "presidential penalty" had the greatest explanatory power. The notion of presidential penalty is the easiest to test in a multicountry data set, as it simply posits that parties that hold the presidency do not fare well in national legislative elections that are held during the term of a copartisan president.

Explanations Based on Divergent Voter Expectations

Another class of explanations for divided government in the United States is based on voters' divergent expectations of legislators and presidents. It should be noted that this class of explanations is not in competition with those based on midterm decline. Indeed, explanations based on *midterm* decline obviously are not intended to explain divided government that results from *concurrent* elections. Explanations based on divergent expectations purport to explain why we sometimes have divided government, regardless of the electoral cycle.

Jacobson (1990) argues that voters simply want different things from their congressional and presidential candidates. Democrats win congressional elections because they offer programs that voters like, but Republicans more often win the presidency because they are judged to be better at ensuring prosperity and security. The problem with this argument is that it appears to rest on peculiarities of the U.S. parties, each of which is identified with specific policies that voters tend to want from one or the other branch of government.[3] Jacobson's argument does not tell us whether this difference in voters' policy expectations is a general feature of presidentialism or is specific to some aspect of the U.S. system. I shall argue that the electoral-rule variable (localizing vs. nationalizing) allows us to explain the phenomenon that Jacobson has identified and generates expectations about how widespread ticket-splitting motivated in this way should be in presidential systems.

Explanations Based on Balancing to Achieve Moderation

Another set of explanations suggests that divided government results from voters' balancing of one party with the other. Examples of this general approach include Fiorina 1992 (pp 67–73) and Zupan 1991, which provide similar arguments about how voters may respond if they desire to break a long period of dominance by one party. It is more difficult to change the majority of the legislature than it is to change the executive in a presidential system, because voters in any given district face a dilemma. If they vote for the out party but voters in a majority of all districts do not, they will deprive themselves of the distributive benefits of having a legislator of the majority party but will not have produced a change in who controls policy. So, instead, they vote for the out party's presidential or gubernatorial candidate, as a check on the actions of the legislature.

Other variants of the balancing argument posit that voters, in splitting tickets and thereby contributing to divided government, are acting as if they favored a more moderate policy outcome than government under the unified control of either party can deliver. Thus, in a time in which the two major parties have located themselves farther from the center of the policy space than most voters desire, ticket splitting and divided government become common (Fiorina 1988). Ingberman and Villani (1993) endogenize the divergent policy preferences of the two parties, hypothesizing that given the institutional structure of the American regime, a party's best way of maximizing its influence over the total policymaking apparatus is to locate away from the center of the policy space. In this way, voters seeking moderation will cast split votes.

Is the balancing hypothesis generalizable to presidential systems outside the United States? In principle, it is, but it will encounter a serious problem in many other presidential systems. As I shall show, situations in which no party holds a majority are common outside the United States, largely because of proportional representation. The balancing models assume that voters are balancing *two* divergent parties. Yet in a system with important minor parties, it is as likely that ticket-splitters are casting their congressional vote for a more extreme party on the same side of the political spectrum as their (more moderate)[4] presidential choice as it is that they are going to the other side of the spectrum to provide "moderation."[5] The argument in favor of moderation between two relatively extreme parties appears to work only in systems in which majorities for one or another party are regularly expected.

JUSTIFICATION AND OPERATIONALIZATION

The timing of elections is operationalized as the share (of unity) of the time between scheduled presidential elections that has elapsed at the time that a given assembly election takes place.

The Electoral Cycle

For concurrent elections, TIME = 0 and for true midterms, TIME = .5. In the data analysis that follows, concurrent elections are nonetheless treated separately from nonconcurrent elections. The following thought experiment will help clarify why concurrent and nonconcurrent elections must be kept analytically distinct.

Suppose that a legislative election would be held the day after the winner of the presidential election is known. In this case, it is possible that there is a reward, rather than a penalty, to the party having won the presidency. If so, then this hypothetical "honeymoon" election should produce the biggest surge in support. Some voters who supported the winner after having been torn between the two front-runners (assuming for the moment that there are just two candidates perceived as being likely winners) would be inclined to switch to the winner. The party of the loser would be left only with its hard-core support.[6] In a concurrent election, on the other hand, even voters who favor a presidential candidate who winds up losing would be expected to support the slate of legislative candidates associated with this candidate, holding constant for the moment possible incentives for ticket splitting. Thus there is a sharp discontinuity between elections at TIME = 0 and those for which TIME > 0. As a result, we can dichotomize elections as concurrent versus nonconcurrent and use the TIME variable to test for differences within the class of nonconcurrent elections.

From the way that the "time" variable is operationalized, the assumption is that it is not so much real time as relative time that matters. We could use real time in months in the regressions presented and get results that are nearly identical. The notion of relative time is theoretically appealing for two reasons, however. First, the model to be tested suggests that the more a president has left of his term, the stronger the incentive is to line up behind the president. Conversely, the model suggests that the later it is in the term, the more jockeying for position for the next term may matter in shaping voters' decisions. For such an effect, it matters less how many months away that election is than whether the legislative election is held with three-fourths of the term left, half a term, or a third of a term. Second (and related to the first reason) the sample of actual presidential terms contains little variation. Most presidential terms are of four or five years. None are shorter than four or longer than seven (see the Appendix). For this reason, in this data set, time measured in actual months and time measured relative to presidential elections are very highly correlated (r = .96). However, there is no reason why terms could not be shorter or longer, thus making the two definitions of time measure very different things.[7]

Legislative Electoral Rules

The premise that there would be any responsiveness at all of assembly elections to the electoral cycle rests on the assumption that voters generally cast their votes on the same basis for the two branches. Under some electoral rules, however, that assumption may be less valid than in others. Let us first consider the logic that undergirds this assumption in the first place; then we shall see under what conditions it may not hold or may hold less strongly.

In endorsing a given presidential candidate, voters are choosing one party label over another, in the process either endorsing the program offered by their preferred candidate or, at least, rejecting that of another.[8] But electing a president is not enough. The president will need legislative support; voters know this, and so they vote for the legislative ticket affiliated with their preferred presidential candidate. If voters behaved in this way, we would always have identical vote totals for parties in presidential and congressional elections, when those elections are concurrent.[9] In nonconcurrent elections, the preferences for or against the president may have shifted over time in the manner already depicted; still, it is on the question of feelings toward the president that voters select their legislative ticket or candidate, according to the theory.

What institutional factors might encourage many voters to behave contrary to this stylized portrayal? One such feature is an electoral process that encourages *local-oriented* voting notwithstanding that what is being elected is a *national* legislative body. For example, where candidates for assembly seats rely on their own initiative rather than the prior endorsement of their parties in order to run for office, they have an incentive to cultivate a "personal vote" based on service to their locality (Cain, Ferejohn, and Fiorina 1987; Carey and Shugart n.d.). The personal vote allows legislators to identify themselves in the voters' minds independently of the national reputation of their party. Even if party organizations control nominations, party decentralization encouraged by some electoral rules may make it difficult for voters to express a vote based on national priorities even if they wish to. In either scenario, we may speak of *localizing electoral rules*, in that the rules create opportunities for legislators to cultivate ties to their local constituencies. Under localizing electoral rules, then, members' personal reputations as good servants for a geographical constituency provide them with voter support independent of or in addition to voters' evaluations of the national party. Given such electoral independence, assembly elections are likely to be less responsive to the cycle of elections and partisan control of the presidency.

Features of electoral rules that may create localizing tendencies are those that inhibit party cohesion and the control of national leaders over locally elected rank-and-file. For example, open primaries on the U.S. model greatly decentralize party control and allow members to tailor their campaigns to their districts. Other countries' electoral laws also have provisions that prevent national party leaders from denying the use of the party name to candidates who use it to suit their own purposes rather than out of loyalty to a party platform, for example, Brazil (Mainwaring 1991); Colombia (Archer 1990, Cox and Shugart n.d.); and the Philippines (Pinckney 1971; Sours 1970). Before a reform was passed in 1958, the Chilean electoral law employed a complex set of procedures that made it difficult for voters to know which candidates (or even which parties, given a frequent tendency of parties to form poorly publicized coalitions) would benefit from their vote (Geddes 1990; Gil 1966; Valenzuela 1994). After 1958, however, the process of registering lists and allocating seats became more transparent (Cruz-Coke 1984); and, largely as a result, national policy came to be more important in campaigns than service to a geographic constituency (Valenzuela 1994). Finally, in Ecuador, a series of party and electoral law provisions encourage loose and uncohesive parties whose members have little connection to any common platform (Conaghan 1994). All of these systems (Brazil, pre-1958 Chile, Colombia, Ecuador, the Philippines, and the United States) are thus coded as having localizing electoral rules. All the other cases (Argentina, post-1958 Chile, Costa Rica, El Salvador, France, Portugal, and Venezuela) are coded as having *nationalizing electoral rules*.

RESULTS

This reasoning leads to two hypotheses: (1) the incidence of divided government will be greater in nonconcurrent than in concurrent elections; and (2)

TABLE 1

Ratio of Presidential to Opposition Majorities in Sole or Lower Houses

ELECTORAL CYCLE	ELECTORAL RULES: NATIONALIZING	ELECTORAL RULES: LOCALIZING	TOTAL
Concurrent	10.0:1	2.3:1	3.1:1
Noncurrent	2.5:1	1.4:1	1.7:1
Total	4.0:1	1.8:1	2.2:1

the incidence of divided government resulting from concurrent elections will be greater if electoral rules are localizing than if they are nationalizing.

The Ratio of Presidential to Opposition Majorities

The simplest means to test these two hypotheses is to compute the ratios of presidential to opposition majorities for each of the combinations of our two variables, the electoral cycle, and the electoral rules. The results, derived from the data displayed in the Appendix, are summarized in Table 1.

We see that presidential majorities occur 3.1 times as often as opposition majorities in concurrent elections, but only 1.7 times as often in nonconcurrent elections. This accords with the first hypothesis. Disaggregating the concurrent elections by nationalizing and localizing electoral rules, we find striking correspondence with the second hypothesis. Presidential majorities occur 10 times as often as opposition majorities in concurrent elections under nationalizing rules, but only 2.3 times as often in concurrent elections under localizing rules.[10] Thus my reasoning appears to be sound.

The Transition to Divided Government

A further check on the argument presented is to see if in systems of localizing electoral rules opposition majorities are more likely to result from a partisan change in the presidency than from a partisan change in the assembly majority. The reason for such an expectation is rooted in the electoral independence afforded by localizing rules. Even if a plurality of voters has chosen to change the partisan composition of the executive branch, enough legislators may survive the adverse presidential outcome to preserve the preexisting majority in the legislature.

In fact, we have nine cases of opposition majorities in systems of localizing electoral rules and just five in systems of nationalizing rules. Table 2 shows that of the nine transitions to divided government under localizing rules, six resulted from a partisan change in the presidency. Four of those cases were in concurrent elections. Only three occurrences of opposition majorities, all from midterms, resulted from a change in the party controlling the assembly.[11] Under nationalizing electoral rules, on the other hand, only one occurrence of an opposition majority resulted from a partisan change in the presidency.[12] Four (all midterm or later) resulted from a partisan change in the assembly.

The findings reported in Table 2, although based on a relatively rare phenomenon, conform to the theoretical expectations. The theory led us to expect localizing electoral rules to grant electoral independence such that even when voters opt to change the partisan control of the presidency, sometimes the legislative majority does not change hands. Under nationalizing electoral rules, on the other hand, the fates of legislators are more closely bound to partisan tides in presidential elections. Also, as expected, change in the legislative majority leads to opposition majorities only in midterm or later-term elections, when voters punish the president's party. No opposition majority has ever resulted from a concurrent election in which voters elected to change the legislative majority but to retain the presidency in the hands of the incumbent party.

Fit to Theory

These findings are consistent with the expectation of the theory presented. Recall that I have suggested that concurrent elections would ordinarily be expected to result in limited ticket splitting. Although I provide no direct data on ticket splitting, the results are consistent with a theory that explains variation in presidential versus opposition majorities by reference to variations in the tendency of voters to vote differently for the two elected branches. When voters are

TABLE 2

Transitions to Opposition Majorities under Different Institutional Configurations

ELECTORAL RULES	PARTISAN CHANGE IN PRESIDENCY	PARTISAN CHANGE IN LEGISLATURE
Localizing	Colombia 1946, Colombia 1982, Philippines 1961,[a] Philippines 1965,[a] United States 1968,[a] United States 1980[a]	United States 1918, United States 1946, United States 1954
Nationalizing	Costa Rica 1966[a]	Chile 1973, El Salvador 1988, France 1986, France 1993

[a] Concurrent election.

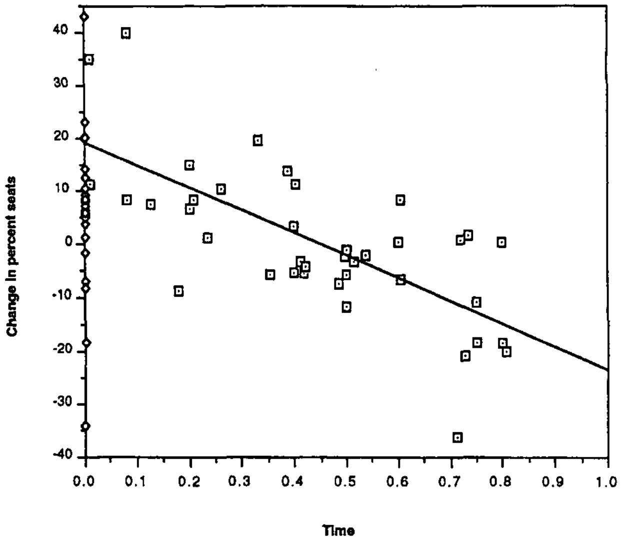
selecting the executive and legislature at the same time, they would be expected to select legislative candidates of the same party as their preferred presidential candidate. Ticket splitting would be higher, however, if legislative elections were conducted to a large degree on the basis of local or personal concerns rather than on support or opposition to a given presidential candidate. The divide between localizing and nationalizing electoral rules in concurrent elections (Table 1) powerfully confirms this line of reasoning. So does the finding that the transition to divided government after concurrent elections is most common under localizing rules and when it is the presidency that changes hands.

THE ELECTORAL CYCLE IN DETAIL

I shall disaggregate the notion of electoral cycle. Instead of considering simply concurrent versus nonconcurrent, we shall look at the specific time at which each assembly election is held relative to a presidential election. Elections occur at nearly every point within a president's term, although few occur very late in a president's term,[13] so that we have plenty of observations.

FIGURE 1

Change in Percent Seats for President's Party over Time



Note: The regression line that is shown is based on elections for which TIME > 0 only, for reasons explained in the text.

Time and Support for Presidential Parties

Figure 1 shows a plot of TIME and change in percent seats.[14] The regression line reflects only the nonconcurrent elections.[15] We find that there is a striking downward trend in seats won by presidents' parties in elections over time. As expected, the coefficient on time is negative and significant. When considering only nonconcurrent elections, this one variable explains nearly 50% of the variance. When concurrent elections are added (as in model 1 in Table 3), the variance explained drops to around 33%, but this is still impressive for a specification that includes only the electoral cycle.

The main reason for the decline in R-squared when including the entire data set is that those systems in which all elections are concurrent add "noise." In such cases, the congressional election that provides us with our observation is being compared with the election held at the time when the outgoing president was elected. A related factor is that concurrent elections (even when the preceding election was a midterm) are by definition held when it remains uncertain who will be the president facing the Congress being elected. In a nonconcurrent election, on the other hand, that president is a known quantity. These considerations provide further justification for considering concurrent and nonconcurrent elections separately, rather than simply coding concurrent elections as TIME = 0.[16]

The relation found between TIME and change in percent votes or seats provides strong support for the "presidential penalty" explanation for midterm loss (Erickson 1988). However, note that there is also a presidential reward in the early part of a term. This finding is consistent with the expectations for honeymoon elections I have articulated. This presidential reward could not have been found in the United States, although the general notion of a presidential honeymoon is widely understood. It takes comparative work such as this to find that the honeymoon is indeed translated into greater voter support of the president's party, given an opportunity afforded by the electoral cycle.

A particularly striking feature of the data plot in Figure 1 is the near intersection of this line with a point that would represent no change in percent seats at the midterm.[17] What this suggests is that in a system in which only midterm elections were used, presidential elections would have virtually no impact on the seat shares of the parties. This is support for the notion of surge and decline, where the decline at the midterm merely represents a reassertion of longer-term electoral patterns following a surge at a concurrent election (Born 1990).

Time and Divided Government

These regressions and data plot show that presidential parties' seats decline as elections are held later in the presidents' term. But what effect does this decline have on the tendency toward divided government? We can answer this question with probit analysis, using an equation specified as follows:

$$\text{OPPMAJ} = a + \beta_1\text{TIME} + \beta_2\text{NUMPAR} + \beta_3\text{USA},$$

where OPPMAJ is a dummy variable equal to one if an opposition party controls a majority of seats, and zero

TABLE 3

Regression Results for Electoral-Cycle Variables and Change in Percent Seats

INDEPENDENT VARIABLE	MODEL 1 ELECTORAL CYCLE VARIABLES ONLY		MODEL 2 WITH ELECTORAL-RULE VARIABLES	
	EST. COEF.	S.E.	EST. COEF.	S.E.
Constant	19.10***	4.34	19.08***	3.49
TIME	−42.73***	8.60	−63.52***	15.20
TIME * LOCAL	—	—	12.13*	6.31
TIME * PRESVOTE	—	—	.32	.28
CONC	−12.32***	4.96	−29.58**	11.02
CONC * LOCAL	—	—	3.16	4.35
CONC * PRESVOTE	—	—	.32	.21
R^2	.33		.49	
No. of observations	64		60	

Note: The dependent variable is change in percent seats for president's party. TIME is the share of the president's term elapsed when the assembly election is held, LOCAL is a dummy set to 1 if electoral rules are localizing, PRESVOTE is the percent of vote received by the president, and CONC is a dummy set to 1 when the election is concurrent with the presidential election.

*$p \leq .10$.

**$p \leq .05$.

***$p \leq .01$.

otherwise; TIME is the electoral cycle variable, as defined earlier; NUMPAR is the effective number of parties represented in the legislature; and USA is a dummy variable for the United States.

NUMPAR is entered as a control variable, simply because majorities of either type (presidential or opposition) are expected to be rarer, the larger the number of parties. The dummy for the United States is included to be sure that an increasing incidence of opposition majorities in elections as TIME increases —if found—is not simply a result of the many U.S. midterm elections that result in divided government. The results are that TIME is a statistically significant predictor ($p < .05$) of the frequency of opposition majorities, even controlling for the effective number of parties and including a dummy for the United States.[18] Thus the probability of an opposition majority increases the later an election is held in a president's term.

Bringing in Electoral Rules and Other Variables

In model 2 in Table 3, I test for a difference in slope on the TIME variable according to the electoral rules. The theory presented suggests that the time and electoral rule variables are interactive. That is, if the electoral independence afforded by localizing electoral rules allows members to shield themselves from evaluations of the party controlling the president, then the slope of the decline according to TIME should be less steep in systems of localizing electoral rules than in systems of nationalizing rules. We can test for this effect by specifying a regression as follows:

$$\Delta\text{SEATS} = k + \text{TIME}\ (\beta_0 + \beta_1\text{LOCAL} + \beta_2\text{PRESVOTE}) + \text{CONC}\ (\mu_0 + \mu_1\text{LOCAL} + \mu_2\text{PRESVOTE}),$$

where ΔSEATS is the change in percent seats for the president's party since the last legislative election; TIME is the electoral cycle variable, as before; LOCAL is a dummy variable that equals one if legislative rules are localizing, and zero otherwise; PRESVOTE is the percent votes received by the president; and CONC is a dummy that equals one if the elections is concurrent, and zero otherwise.

By making the variables interactive, this specification lets us test for the hypothesis that the slope of the line for TIME is significantly different according to which type of legislative electoral rule is used. If this hypothesis holds, the coefficient on TIME will remain negative and significant and the coefficient on TIME * LOCAL will be positive and significant, indicating that the decline over time is less steep for localizing rules than for nationalizing rules. If concurrent elections also vary according to electoral rule type, then CONC should remain negative and significant, while CONC * LOCAL should be positive and significant.

Further, I expect that the standard errors on TIME * LOCAL and CONC * LOCAL will be larger relative to their estimated coefficients than will be the standard errors on TIME and CONC. The reason is that the sheltering from the cycle effect afforded by localizing rules is only facilitating and potential; members are still subject to varying degrees of reward or penalty according to TIME when their party holds the presidency. If they were not, then TIME would have no effect at all in systems with localizing rules; at the extreme, perhaps we should indeed expect no effect of TIME. Realistically, however, we should expect the effect of evaluations of the president only to be dampened—and possibly to varying degrees in different elections and for different legislators. Again, the reason is simply that localizing rules provide the *opportunity* for members to shelter their fates from evaluations of the president. On the other hand, I have argued that under nationalizing rules, legislators' fates are inextricably bound to voters' evaluations of a president from the same party. Therefore, the standard errors on TIME and CONC should be much smaller relative to their estimated coefficients than for the terms that are interacted with LOCAL.

PRESVOTE, the percent of votes won by the president, is included and also interacted with the cycle variables because a president who has won a larger "mandate" may experience a greater late-term loss. This expectation is simply a generalization of the "regression to the mean" explanation for varying degrees of midterm losses seen in the United States (e.g., Hinckley 1967). The coefficient on TIME * PRESVOTE might be negative, so that a larger share of votes for the president portends greater-than-average penalty later in the term. The coefficient on CONC * PRESVOTE, however, should be positive, as a president elected with larger shares of the vote ought to have more "coattails" in a concurrent election.

The results of this regression are displayed in Table 3 as model 2.[19] The expectations on the electoral rule variables are largely confirmed. The coefficient on TIME indeed remains negative and highly significant ($p < .01$), and the coefficient on TIME * LOCAL is positive and significant ($p < .1$, almost $p < .05$). The standard error on TIME * LOCAL is indeed larger relative to its coefficient than the standard error on TIME.[20] CONC also remains negative and significant. The coefficient on CONC * LOCAL is of the expected sign but is statistically insignificant. In part, this is because (as was discussed in connection with model 1), there is already a lot more scatter on CONC than on TIME, because the reference point for ΔSEATS is often a full presidential term in the past.

The estimated coefficients on the two interactive terms involving the president's vote are utterly insignificant. Indeed, the coefficient on TIME * PRESVOTE is of the wrong sign as well. Model 2 thus suggests that the electoral cycle and electoral rule variables are more important in predicting the change in percent seats held by the president's party than is the share of votes won by the president.

The Electoral Cycle, Electoral Rules, and the Party System

So far, I have dealt with two tasks. First, we saw how the ratio of presidential to opposition majorities varied with different institutional configurations. Then we saw the effects of presidential reward in early-term elections and presidential penalty in elections held later in the term. Our task is now to expand our scope to include an important aspect of the relationship between party systems and majority situations—the phenomena of multipartisan and *no-majority* situations. Such situations are actually the second most common in presidential systems, according to the data presented in Table A-1, in the Appendix. No-majority situations account for 29% of all observations, compared to 49.1% of cases in which the president has a majority and 21.9% in which an opposition party or alliance has a majority.[21] I seek to determine whether the occurrences of no-majority situations are randomly distributed across the cases or depend on institutional variables in any systematic way. We shall see that localizing elections appear to reduce the number of parties in concurrent elections. However, the best predictors of increased number of parties (and therefore of no-majority situations) are district magnitude and, especially, the combination of nonconcurrent elections with localizing rules.

Let us consider the effects of localizing electoral rules and how such rules might operate differently on the number of parties in systems that use concurrent elections versus systems that use (exclusively) nonconcurrent elections. If the party label can be adapted to the needs of local candidates and incumbent legislators (as it can under localizing rules), then there is reduced incentive to establish separate parties when disagreements arise between local chapters and the national leadership. When elections are concurrent, the focus of the campaign is likely to be on the nationwide race for the presidency. Thus candidates who can freely choose their party label or mold it to suit local concerns have the incentive to line up behind one or the other presidential candidate,[22] rather than behind a party that will not attract much attention in the presidential race. At least as long as the election for president is conducted under plurality or pluralitylike rules,[23] ordinarily this logic would imply just two principal parties (Shugart and Carey 1992, chap. 10). Other things being equal, then, third parties should be less common under the combination of localizing electoral rules and concurrent elections with plurality (or pluralitylike) electoral rules for the presidency. Thus the way in which this combination structures incentives of members of legislatures and candidates to the legislature should make majorities—whether presidential or opposition—likely. In nonconcurrent elections, on the other hand, the absence of the presidential contest to structure the party system implies that there may be no adverse cost to candidates' being associated with third parties (Shugart and Carey 1992). There may even be some benefit to *not* being identified with the president's party, given the decline observed in midterm or late-term elections (albeit a dampened one, as model 2 in Table 3 showed).[24]

To summarize, the combination of concurrent and localizing elections leads to an expectation of a two-party system and frequent majorities (for either the president or the opposition). The nonconcurrent–localizing combination would suggest a tendency toward multipartisan and no-majority situations. To see if these expectations hold, we can perform two statistical tests, one of which appears to support this reasoning and the other of which casts some doubt on it. As I shall discuss, there are reasons having to do with the sample of cases to accept the logic of my hypothesized effect of concurrent elections and localizing electoral rules. The two statistical tests are (1) a regression of various institutional variables on the effective number of parties and (2) a probit analysis of these same variables on the probability of legislative majorities.

The first test uses ordinary least squares regression analysis in an equation specified as follows:[25]

$$\text{NUMPAR} = a + \beta_1\text{CONC} + \beta_2\text{LOCAL} + \beta_3\text{NCLOCAL} + \beta_4\text{MAG},$$

where NUMPAR is the effective number of parties in the assembly; CONC is a dummy variable equal to one if the election is concurrent, and zero otherwise; LOCAL is a dummy variable equal to one for localizing electoral rules, and zero otherwise; NCLOCAL is a dummy variable set to one for localizing electoral rules *combined with* nonconcurrent elections, and zero otherwise; and MAG is the magnitude (number of seats per district) of the electoral system.[26]

If my explanations are valid, then CONC and LOCAL should have negative and significant coefficients, because concurrent elections should encourage voters to support the larger parties that have a chance at the presidency and because localizing electoral rules should allow parties to be affiliated with a major-party presidential candidate and simultaneously loose enough to let members respond to local concerns. NCLOCAL should be positive and significant. Of course, MAG should be positive and significant.

Table 4 shows the results. CONC and LOCAL are both of the expected sign, though the coefficient on LOCAL ($p < .1$) is less significant than the coefficient on CONC ($p < .05$). Surprisingly, district magnitude is not significant. The results suggest that when elections are concurrent, the effective number of parties is reduced from 3.2 on average (the constant term) to around 2.4. When electoral rules are localizing, it is reduced by a further .72 effective party (the coefficient on LOCAL). Thus, when both CONC and LOCAL are set to one, the average effective number of parties is predicted to be about 1.6, which is a value that obtains when there are just two major parties with one holding a substantial majority. The insignificance of magnitude in this equation is surprising, given that magnitude has been called the "decisive factor" in determining the number of parties (Taagepera and Shugart 1989). However, most studies of the effects of electoral systems have dealt overwhelmingly with parliamentary systems. The finding here confirms that presidential systems should be analyzed in their own right and not as though they were just like parliamentary systems, other than their manner of executive selection. The reductive effect of concurrent elections on the number of parties (at least when a pluralitylike rule is used to elect the president) is such that even large-magnitude presidential systems have a low number of parties. To cite a couple of specific examples, both Costa Rica and Venezuela have fairly high effective magnitudes (8 and 27, respectively), yet both have fairly low numbers of legislative parties (well under three in most elections), in part because of concurrent elections and two-candidate competition for the presidency in most contests.

TABLE 4

Effective Number of Parties in the Legislature and Probability of Legislative Majority

	EFFECTIVE NUMBER OF PARTIES		PROBABILITY OF LEGISLATIVE MAJORITY	
INDEPENDENT VARIABLE	EST. COEF. (OLS)	S.E.	EST. COEF. (PROBIT)	S.E.
Constant	3.18***	.29	.85**	.33
CONC[a]	−.82**	.36	−.23	.43
LOCAL[b]	−.72*	.40	.67	.52
NCLOCAL[c]	1.64***	.49	−1.45**	.61
MAG[d]	.03	.02	−.06**	.03
R^2	.38		—	
% correctly predicted	—		77.5	
No. of observations	71		71	

[a] Concurrent election.
[b] Localizing electoral rules.
[c] Noncurrent election with localizing electoral rules.
[d] Effective magnitude.
*$p \le .10$.
**$p \le .05$.
***$p \le .01$.

Thus we can say that concurrent elections reduce the number of parties and that the combination of concurrent elections and localizing electoral rules tends to reduce the number of parties to two. This is consistent with the expectation that this configuration would give legislative candidates the advantages of being aligned with a major presidential contender, but also the electoral independence to tailor their message and behavior to suit the needs of local constituents. Next we test whether the CONC + LOCAL combination also produces a greater probability of majorities.

Table 4 also shows the results of a probit equation with the same independent variables. The dependent variable is MAJ, a dummy variable that equals one if any party has a majority of seats. The coefficients on CONC and LOCAL are of the expected signs but (especially CONC) far short of being significant. Magnitude is significant in this equation ($p < .05$).

What accounts for the contradictory result? Although presidential majorities are common with concurrent elections, no-majority situations are so common as well that there is no statistically significant effect of concurrent elections alone on the probability of majorities. That is, the number of parties may be reduced by concurrent elections but not enough that majorities become significantly more probable for either the president or the opposition. Although this equation suggests that magnitude is a stronger factor in predicting majorities, there are reasons based both on the sample of cases and findings already demonstrated not to reject the hypothesis that localizing rules in concurrent elections are also a significant factor. First, in the data sample, two countries that contribute a number of observations, the Philippines and the United States, both use low magnitude (M = 1) in addition to localizing electoral rules.[27] Because these cases (plus Colombia, where localizing electoral rules are combined with relatively high magnitude and near-concurrent elections) are the only ones that consistently have majorities for one party regardless of the value of TIME, the probit analysis on a dichotomous dependent variable naturally picks up magnitude as the more important factor. However, the regression that has a continuous dependent variable (effective number of parties) has already shown us that CONC and LOCAL are more significant predictors of a reduced number of parties than is magnitude. Moreover, model 2 in Table 3 showed us that in general, localizing electoral rules dampen the degree of responsiveness of the electoral cycle to the partisan control of the presidency. These findings provide some reason to suspect that if we had a sample in which there were more cases of localized electoral rules and concurrent elections with magnitude greater than one, we might find a greater probability of a majority, just as we find a reduced number of parties. As it is, however, the two models presented in Table 4 alone do not sort out fully the effects of the different variables.

A further justification for not abandoning the reasoning that localizing electoral rules are an important variable in their own right in contributing to majorities (including opposition majorities) can be seen by reference to Table 2. In this table we saw that six of seven transitions to divided government resulting from a partisan change in the presidency—but only three of seven transitions resulting from a partisan change in the legislative majority—occurred under localizing electoral rules. The table also showed that five of the seven transitions occurring from a partisan change in the presidency resulted from concurrent elections, while all the transitions resulting from a partisan change in the legislature occurred in midterm elections.

If the electoral system's magnitude were a more important cause of majorities, we should not expect such patterns. Low magnitude contributes to majorities not only by reducing the number of parties but also by producing "manufactured" majorities—those in which a party did not have a majority of votes but won a majority of seats (Lijphart 1994; Rae 1967). If low magnitude were a more important predictor of opposition majorities than are localizing electoral rules, then we should expect some concurrent elections to produce divided government from a change in the partisan composition of the legislature (i.e., there should be some cases of concurrent elections in the right side of Table 2). Particularly in close elections, a low-magnitude electoral system would from time to time give the legislative majority to a party different from that which won the presidency—possibly even different from that which won the most legislative votes. Instead, we find that opposition majorities that result from concurrent elections occur under localizing electoral rules and from a partisan change in the presidency. This was the pattern that was predicted on the basis of a theory that postulated that voters in systems of localizing electoral rules would sometimes vote to change the presidency but would find that their local representatives had served them well enough that they would split tickets.

Moreover, another result in Table 2 supports my contention that concurrent elections and localizing rules are more important than magnitude for both reducing the number of parties and predicting divided government. If, as I have argued, voters are more interested in and capable of punishing the president's party in midterm elections under nationalizing rules than under localizing rules, then magnitude should matter relatively less under nationalizing rules. That is, while a very high magnitude would make it unlikely that a single party would claim a majority, voters' focus under nationalizing rules on the performance of the governing party would produce opposition majorities after midterm elections in medium-magnitude as well as low-magnitude systems. Under localizing rules, on the other hand, the greater insulation of legislators should mean that only in low-magnitude systems (with their greater tendency to magnify small changes in votes percentages into relatively larger changes in seats percentages) would we see changes in partisan control of the legislature at midterm. Indeed, very low magnitudes do not appear to be required to turn the partisan control of the legislature against the president in midterm elections if the electoral rules are nationalizing. Of the four cases of a transition to divided government resulting from a partisan change in the legislature under nationalizing rules, only one (France 1993) occurred under single-member districts. On the other hand, all three cases of transition to divided government resulting from change in the party controlling the legislature under localizing rules occurred with single-member districts—and all in the United States.

This discussion of effects of electoral cycle and nationalizing or localizing electoral rules has suggested ways in which the incidence of divided government may be affected by the impact of these variables on the party system. Although we have too few cases of opposition majorities to draw firm conclusions, I have presented an argument about why divided government would be more likely to result from (1) concurrent elections in which localizing electoral rules are used and (2) midterm or later elections, especially those in which nationalizing rules are used.

Testing the Balancing Hypothesis

As noted previously, Ingberman and Villani (1993) have a model in which divided government results from parties' taking divergent policy stances and voters' preferring moderation. Given the president's veto, both parties must compromise to enact policy under divided government. I shall test this hypothesis on the present data set. I have identified the conditions under which a two-party system is most likely in presidential systems—concurrent elections (with plurality presidential elections) and localizing electoral rules. Two-partism is crucial to Ingberman and Villani's argument, as well as to any other that posits balancing as an explanation of divided government, because it assumes that one party or the other will win a majority of legislative seats. The veto is also crucial, as a president who lacks a veto cannot "moderate" the policy output of the legislature. Many presidents do not wield a veto; many who do confront multiparty systems.

A probit equation is specified:

$$\text{OPPMAJ} = a + \beta_1\text{VETO} + \beta_2\text{NUMPAR},$$

where OPPMAJ is the dummy for whether or not there is an opposition majority, VETO is a dummy equal to one if there is a presidential veto requiring at least a two-thirds vote to override, and NUMPAR is the effective number of assembly parties. If balancing is a valid explanation, VETO should have a positive and significant effect. That is, opposition majorities are certain to be more common in two-party systems than in multiparty systems, simply because either kind of majority is more common. But if balancing as a result of the veto is a predictor of divided government, then VETO should be a significant predictor of the probability of OPPMAJ beyond that given by NUMPAR.

However, VETO is not significant, holding NUMPAR constant, whether or not a dummy variable is included for the United States. In both specifications, TIME and NUMPAR remain significant even when the effects of a presidential veto are considered.[28] Thus I find no support in a data set on comparative presidential systems for the balancing hypothesis that has been developed with respect to the specific conditions of the United States.

DISCUSSION

The results of this article allow us to place the study of divided government in the United States into a broader comparative perspective than has generally been attempted. The evidence from 12 countries suggests that the electoral cycle is of great importance in determining whether presidents enjoy legislative support in the form of a copartisan majority or face an opposition majority, as well as in the growth or decrease in that support over time. Legislative elections that are concurrent with presidential elections[29] are more likely than those that are nonconcurrent to produce unified government. Nonconcurrent elections, other than those held early in the president's term, are more likely to produce divided government. This is a generalization of the on-year/off-year pattern in the United States that was especially pronounced before 1956.

Other major results include the following. Elections held early after a presidential election are likely to produce a surge in support for the new president's party, with a simple linear model fitting the data and predicting a decline over time in elections held later in the term. Electoral rules that facilitate legislators' cultivation of ties to local constituencies[30] reduce the degree of decline in support for presidential parties over the course of an electoral cycle. Such independence of legislators from party also may make for a greater likelihood of divided government even in concurrent elections, as voters who opt for one party for the presidency may find that a candidate of another party has served local interests well. Thus, while presidential majorities are more common in concurrent elections, the electoral rules variable is also a significant predictor of the number of parties in concurrent elections. The reduction in the number of parties that is associated with localizing electoral rules in concurrent elections may account in part for the incidence of divided government in United States and Philippine concurrent elections.

One question that arises is, If the tendency toward opposition majorities is inherent in the institutional design of the United States (and some other countries), why has divided government in concurrent elections been almost entirely a recent phenomenon in the United States? There are two responses. First, a more sophisticated measure of the degree of electoral independence than the dichotomy used here might capture differences over time in the United States. States adopted primaries at different times and have changed the degree of "openness" of primaries over time. At least through the nineteenth century and into the early twentieth, the degree of party control (at least at the state level) was no doubt greater than it has been in the post–World War II era, when members have increasingly been able to cultivate a "personal vote" at the level of their own districts (Cain, Ferejohn, and Fiorina 1987). Second, nothing in the theory articulated here implies that localizing electoral rules guarantee the divergent voter expectations that may produce divided government. Such electoral rules do, I agree, make the articulation of such divergent preferences easier than do other institutional formats. And I further claim that nationalizing rules practically guarantee that legislative elections reflect voters' evaluations of the president. The data analysis supports this idea strongly.

There is a need for much more systematic research on the *consequences* of divided government. As noted in the introduction, there is some dispute about its supposedly negative effects in the United States. In the comparative presidentialism literature, on the other hand, there is no dispute at all about the "perils" (Linz 1990) of a lack of presidential majorities, let alone opposition majorities. As far as the United States is concerned, Mayhew's (1991) excellent study calls into serious question the claims made in the literature (both American and comparative) about the perniciousness of divided government. He cites electoral incentives of members of Congress as one plausible reason for the lack of a serious divergence between unified and divided government in patterns of either congressional investigation or the passing of major legislation. The present analysis provides support for Mayhew's explanation, as in systems with localizing electoral rules, members of the legislature are much less tied to the collective fates of their parties than in other systems. Thus members are more self-reliant in building their careers on legislative or constituent-service-oriented reputations. The resulting less-cohesive parties imply that executive-legislative relations would be less sensitive to which party controls the branches under localizing electoral rules. In systems with nationalizing electoral rules, on the other hand, divided government may be much more difficult to cope with, as parties may have a stronger collective incentive to accentuate their differences.

To return to themes about divided government versus no-majority situations, we can draw some tentative conclusions about the effects of different patterns of nonunified government on the performance of presidential government. If parties are less cohesive (as I have argued they are likely to be under the combination of concurrent elections and localizing rules), then divided government may not in fact be a form of grand coalition. Rather, less cohesive parties imply that much policy would be made by one cross-party coalition opposing another cross-party coalition. In such a situation (which appears to fit the experience of the United States, and probably the Philippines and Colombia as well), party lines may not be the best predictors of policy stance; but it may not be accurate to charge that nobody is responsible for policy because everyone is responsible, as criticism of American divided government in the popular media often suggests. It is, however, undoubtedly valid to say that determining this responsibility is a much tougher task for voters, because it is necessary to look beyond the party label into the record of individual politicians.[31]

On the other hand, if parties are cohesive and nationally focused (as I expect them to be under nationalizing electoral rules), divided government may mean de facto grand coalitions or even crises of "dual legitimacies." That is, under such party systems, cross-party alliances would be much harder to craft and governance might indeed be made more difficult unless there were other constitutional rules to mitigate such stalemates, such as the French provision that control over the government is ceded to the legislative majority when it is different from the party of the presidency (Pierce 1990; Shugart and Carey 1992; Suleiman 1994).[32]

Opposition majorities are not unique to the United States, but they also are not the norm in other systems that elect their chief executives directly. They are the product of specific institutional configurations about which there is considerable variation cross-nationally.

APPENDIX

Case Selection. Table A-1 shows the countries and time periods included in this study, along with the lengths of presidential and assembly terms and information on electoral rules and cycles. The table also indicates the number of assembly elections from which observations were taken and gives the percent of these observations that produced each of the three possible majority situations (presidential, opposition, or no majority). Cases were selected as follows. First, a country had to have a popularly elected president. Where elections are indirect, a country was included only if the electors who make the final selection must vote only once and therefore are unable to broker the outcome. If the legislative assembly sometimes make the final selection from among candidates who first ran in a direct popular vote, the country was included. Thus Argentina, Chile, and the United States are all included because no candidate who won a plurality of the popular vote has been denied the presidency in the time of the study, but Finland is excluded.

Second, voters had to be free to cast their presidential and legislative votes for different parties. Thus Bolivia, the Dominican Republic, Honduras, and Uruguay are excluded, as voters in most or all of the elections in these countries have been required to vote a straight ticket.

Third, the president must exercise real political powers. Such powers could be legislative (veto or decree) or nonlegislative (e.g., appointing cabinet ministers, dissolving parliament); but where the secondary literature indicates that presidents do not exercise such powers as they are formally granted, the case was excluded (Duverger 1980; Nogueira 1986; Shugart and Carey 1992). Thus the data set does not include Austria, Iceland, or Ireland.

No fewer than three elections are used for each country. Where a country changed its institutional configuration between elections, it is given multiple entries in Table A-1. The table shows the variety of term lengths used in the countries studied. It also indicates whether a country's constitution permits elections for either branch to be called ahead of schedule. For instance, in Chile, France, and Portugal, there is no vice-president, so if a president dies or otherwise vacates the office, a new election is held for a new full term. In France and Portugal, presidents may dissolve parliament, thereby ordering early elections. The values of the variable TIME indicate the share of the scheduled presidential term that had elapsed at the time an election was held. That is, even if a president does not complete the term for which he was elected, an election held while that president is in office can be assumed to reflect the portion of the planned term that had elapsed. All that is required to accept this assumption is for presidents not to announce in advance of legislative elections that they plan to retire early.

TABLE A-1

Summary of Data

Country	Years in Data Set	Presidential Term		Assembly Term		Electoral Rules[a]	Effective Magnitude	Electoral Cycle	N	% of Observations with Majority for . . .		
		Years	Early Elections?	Years	Early Elections?					Pres.	Opp.	Neither
Argentina	1983–89	6	no	4[b]	no	National	6.2	Mixed	5	40.0	.0	60.0
								Concurrent	2	50.0	.0	50.0
								Nonconcurrent	3	33.3	.0	66.7
Brazil												
I	1945–50	5	no	5	no	Local	12.5	Concurrent	2	50.0	.0	50.0
II	1954–62	5	no	4	no	Local	12.5	Nonconcurrent	3	.0	.0	100.0
III	1990	5	no	4	no	Local	19.3	Nonconcurrent	1	.0	.0	100.0
Chile												
I	1937–57	6	yes	4	no	Local	4.0	Nonconcurrent	6	.0	.0	100.0
II	1958–73	6	yes	4	no	National	4.0	Nonconcurrent	4	25.0	25.0	50.0
Colombia												
I	1945–49	4	no	2	no	Local	8.7	Nonconcurrent	3	33.3	66.7	.0
II	1974–91	4	no	4	no[c]	Local	7.7[d]	Nonconcurrent[e]	6	83.3	16.7	.0
Costa Rica	1953–90	4[d]	no	4[f]	no	National	8.1	Concurrent	10	60.0	10.0	30.0
Ecuador	1979–90	4[e]	no	2[g]	no	Local	2.8	Mixed	5	.0	.0	100.0
								Concurrent	3	.0	.0	100.0
								Nonconcurrent	2	.0	.0	100.0
El Salvador	1985–91	5	no	3	no	National	4.9	Nonconcurrent	3	33.3	33.3	33.3
France	1967–93	7	yes	5	yes	National	1[h]	Nonconcurrent	8	50.0	25.0	25.0
Philippines	1953–69	4	no	4	no	Local	1	Concurrent	5	60.0	40.0	.0
Portugal	1979–87	6	yes	4	yes	National	12.0	Nonconcurrent	5	60.0	.0	40.0
United States	1912–90	4	no	2	no	Local	1	Mixed	41	61.0	36.6	2.4
								Concurrent	21	66.7	28.6	4.8
								Nonconcurrent	20	55.0	45.0	.0
Venezuela	1958–88	5	no	5	no	National	27	Concurrent	7	42.9	.0	57.1

[a] Localizing or nationalizing, as explained in text.
[b] Staggered terms, with one-half renewed every two years.
[c] One early election called to replace congress according to new constitution of 1991.
[d] 4.9 in 1991.
[e] Concurrent in 1974.
[f] Five-year term after election of 1953.
[g] Four-year term for deputies elected from nationwide district, five years for all deputies and president elected in 1979.
[h] 5 in 1986.

Electoral Rule Variables. Electoral rules were coded on a dichotomous variable according to whether they were nationalizing or localizing in their effects on legislative campaigns. If legislative aspirants may not run for office except as candidates of a legally registered party and if voters cast votes for party lists, as in Costa Rica and Venezuela, rules are nationalizing. If, on the other hand, party names are either absent from the ballot or may be adopted by candidates on the basis of their own entrepreneurial activities—paying a deposit or collecting signatures of voters—then electoral rules are localizing because such candidates have more freedom to tailor their campaigns to local concerns. For example, in the United States, candidates receive the right to run under the label of a party by winning a primary contest. Nor do parties in Colombia (Archer 1990) and the Philippines (Pinckney 1971; Sours 1970) control the use of their labels.

In Brazil, local party organizations can admit a candidate onto the party list without the approval of the state-level party, let alone the national party organization. Moreover, once elected, a legislator cannot be denied the use of the party label in the future (Mainwaring 1991). The change in electoral rules in Chile after 1958, from localizing to nationalizing, was discussed in the main text.

Effective magnitudes are determined using the method discussed in Taagepera and Shugart (1989, 126–35, 266–69), which takes into account varying magnitudes of individual districts in some countries, as well as thresholds. For electoral systems in which all seats are allocated in districts, effective magnitude (M_{eff}) is calculated as

$$M_{\text{eff}}=E/S, \tag{1}$$

where E is the number of electoral districts and S is the number of seats in the legislature. For electoral systems with thresholds, we use the formula

$$M_{\text{eff}}=50\%/T, \tag{2}$$

where T is the percent of votes required for a party to win a seat. The Venezuelan electoral system is highly complex, and we suggest and justify using the geometric average of the values of effective magnitude that result from equations 1 and 2 (ibid., 269). We provide values for several countries included here (ibid., 136–37). For most others, our methods are applied to the descriptions in Nohlen 1993. Effective magnitude in this data set ranges from 1 to 27. The mean magnitude in the entire data set is around 7; the median is around 4.

Votes and Seats Data. Data on votes and seats won by presidential parties are from Mackie and Rose 1992 and Nohlen 1993. For the Philippines, data are from the *Report of the Commission of Elections to the President of the Philippines and the Congress*. A party was considered to be aligned with the president only if the sources indicated that the party had endorsed the president at the time of the presidential election (the first round, in the case of a two-round electoral system). In most cases, only a single party was so listed. If several party organizations listed themselves on the ballot as a single alliance (as in Chile in 1973 and France in 1986), the alliance was counted as a "party."

Dealing with U.S. Observations. The one country from which there are especially numerous observations is the United States. Accordingly, for the regressions (but not for Tables 1 and 2), to avoid having this one case swamp the results, the number of observations was reduced to nine. Five consecutive elections were chosen from 1924 to 1932 and four more from 1980 to 1986. The results on the TIME variable from these elections were checked against those from the entire set of U.S. elections (1912–88) to ensure that the elections chosen were not atypical. They were not. For example, in all U.S. observations, the president's party, on average, gained 2.8% votes in the on-year election and lost 3.8% at the midterm. In the smaller sample, the figures were an on-year surge of 3.9% and an off-year decline of 2.5%. Given standard deviations of greater than 2, the difference is not significant.

Upper and Lower Houses. All data are from lower houses. We might expect the pattern of divided versus unified government to be different in upper and lower houses, perhaps because the two houses are elected on a different basis or because staggered elections are used in the upper houses. In fact, the overall rates of divided government are almost identical in upper and lower houses. For example, presidents held majorities in 36.8% of lower-house observations and in 39.1% of upper-house observations. Opposition majorities occurred at rates of 15.8% and 13.0%, respectively. This similarity results partly from the rarity, except for the United States, of presidential systems in which the electoral system or districting arrangements for the upper house differ much from those of the lower house. By using only lower houses, we avoid multiplying the number of observations for some countries, but not for others that happen to be unicameral. There are 23 elections in unicameral systems in the data set, plus eight elections from France, which for present purposes may be considered effectively unicameral, given the weakness of its Senate (Lijphart 1984). That makes a total of 31 effectively unicameral observations. There are 38 lower-house elections in the data set from countries with what Lijphart (ibid.) calls symmetric bicameralism (leaving the numerous U.S. observations aside for now). Adding upper houses would have added an additional 46 elections, but all from countries already covered. (Upper-house elections, for at least some seats, occur more frequently in many countries than do lower-house elections.)

Veto Variable. A president is coded as having a veto only if a vote of at least two-thirds of the legislators is required to override it. In systems in which some types of legislation have different override requirements than others, the coding depended on what the veto override procedure is on the more important legislation, such as budgets. For example, the Costa Rican and Ecuadorian presidents have vetoes with at least two-thirds required to override on all legislation *except the budget*. Thus Costa Rica and Ecuador are scored as having no presidential veto. In Colombia before 1991, the situation was reversed, with the stronger veto being on most types of expenditure.

Legislators' Party Switching. Among the Philippine legislatures in the period of study, it was common for members to be elected under the label of one party but subsequently switch to another party (Sours 1970). The same is currently true in Brazil (Mainwaring 1991) and Ecuador (Conaghan 1994). In all cases, the label that a candidate bore in the election (i.e., the one revealed to voters) is the one that is counted. Changing party affiliation once elected is not of concern here. Thus my data show episodes of divided government in the Philippines, even though presidents of the previously "out" party were generally able to entice sufficient defectors from the erstwhile majority party to cross party lines and form a congressional majority.

Notes

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1. This vast literature cannot be reviewed thoroughly here. General studies of divided government include Cox and Kernell 1991; Fiorina 1992; and Jacobson 1990.

2. One twentieth-century concurrent election, that of 1916, had left neither party with a majority of seats.

3. Fiorina (1992) makes a similar point about another form

of comparative study of divided government—that focused on the states.

4. For the sake of illustration, I am assuming that there are only two parties with a realistic chance of winning the presidency and that they are competing for the median voter (Downs 1957). In the stylized proportional representation congressional election, however, there are more parties, some of which cater to more extreme policy positions.

5. To illustrate this argument, suppose the United States had several minor parties that stood to win congressional seats in addition to the Democratic and Republican parties. In such a case, some voters who currently vote straight tickets would become ticket-splitters (e.g., some who voted Democratic for president might vote for a Rainbow Coalition legislative slate).

6. This intuition is recognized by some who advocate reform of the United States system. Robinson suggests that congressional elections be held one month after presidential elections (when TIME = .021) in order to increase the probability of presidential majorities (1985, 117–19).

7. Indeed, until recently, some U.S. governors served for only two-year terms. The Chilean constitution of 1980 called for eight-year terms. If the effects of time are to be generalized, term length could make a large difference at the extremes. Might the choice of relative or absolute time depend on whether one posits that the electoral cycle effect stems from voter or elite calculations? An explanation based on voters may imply absolute time, as voters update their perceptions of the president's party based on new information upon which to base retrospective voting. An explanation based on elites may imply relative time, as elites look ahead to the remaining time in the term and thereby shape election outcomes. Because of the high correlation between absolute and relative time in the data, it is impossible to sort out these intriguing possibilities. However, this reasoning may help drive future modeling of the effects of timing of elections.

8. I add *rejecting that of another* to take account of the notion that voters may seldom vote with the idea of conferring a "mandate." Rather, they may vote retrospectively (Downs 1957; Fiorina 1981). For a very useful review of these perspectives, see Powell 1989.

9. Such a theory would leave no room for ticket splitting. However, increasing district magnitude (M) in the assembly election would predict greater ticket splitting, even if all voters were motivated by national policy priorities. Some voters would vote for a minor party for Congress but vote strategically between the two viable contenders for the presidency, for which $M = 1$. For detailed models with data, see Shugart 1988 and Shugart and Carey 1992.

10. To be sure that the results were not being swamped by the large number of observations from the United States, I checked the ratios with those observations excluded. The spread between the ratios of presidential to opposition majorities actually widens—4.7:1 for concurrent elections and 2.3:1 for nonconcurrent. The ratio in concurrent elections under localizing rules barely changes (2.0:1). The one result that is affected substantially is the ratio for nonconcurrent elections under localizing rules, which is identical to that seen for the concurrent elections (2.0:1). This is because the two-party system in the United States results in many more cases of presidents retaining their majorities even after (typically) losing seats at midterms than do the more typical multiparty systems elsewhere.

11. Is the United States in 1930 an additional occurrence? In that election the voters returned a majority of Republicans in the House of Representatives (while the Republicans continued to control the presidency), but by the time Congress convened, deaths and Democratic by-election victories gave the Democratic party a majority. As this study concerns itself with election day outcomes, it is reasonable to classify this case as one in which unified government was maintained by voters, albeit precariously.

12. The one case with opposition majority resulting from a change in the presidency in a system of nationalizing electoral rules was a close call: in Costa Rica in 1966, the party with the legislative majority lost the presidency 50.5% to 49.5%.

13. Colombia is the one country that regularly uses very-late-term elections, held about two months before the next presidential election. These elections have been excluded from the regressions on cycle effects because they are less likely to be influenced by evaluations of the incumbent than they are to serve as part of the campaign for the successor. Indeed, Jones (1994) has classified the Colombian cycle as neither concurrent nor nonconcurrent, because while the elections are technically nonconcurrent, the *terms of office* are concurrent.

14. I have used seats rather than votes because what we are ultimately concerned with here is what party, if any, controls a majority of seats. The results are similar if we use votes (not shown for reasons of space) but less pronounced, owing to the tendency of nearly all electoral systems to give some bonus in seats to the party with the most votes (Rae 1967).

15. I have already justified the separate treatment of concurrent and nonconcurrent elections.

16. What about coding concurrent elections as TIME = 1, based on the outgoing president? I tried such a specification, and the results were unsatisfactory. Concurrent elections are therefore different in kind, not just in degree, from nonconcurrent elections.

17. The actual figures for TIME = .5 are −2.3% for seats. The corresponding figure for votes is −1.3%.

18. The coefficients (and standard errors) are (1) a constant of −.16 (.66), (2) 1.22 (.56) on TIME, (3) −.44 (.22) on NUMPAR, and (4) .80 (.52) on the U.S. dummy. If we exclude the USA dummy, NUMPAR becomes more significant and TIME remains significant. Thus it is the impact of the effective number of legislative parties, not that of TIME, that is weakened by the U.S. observations.

19. Other specifications were run with dummy variables for several countries for which there were many more observations than for others or that appeared to be possible outliers. None of these country dummies was significant and none had a more than negligible effect on the coefficients of the other variables. I also attempted to test for effects of economic growth, expecting that the sensitivity of seat shares to the electoral cycle might be affected by economic performance. However, data are available for most of these countries and time periods (if they are available at all) only on an annual basis, while what is needed is performance over the interval between elections. I tried several specifications and none produced a significant coefficient on the economic variable.

20. The t-score on TIME is −4.18. On TIME * LOCAL, it is 1.92.

21. With the United States excluded, the percentage of no-majority situations is much higher, 43.8%, but mainly because of the far less frequent occurrence of opposition majorities outside the United States (only 13.7%). Presidential majorities are only slightly less common outside the United States than in the United States—42.5%.

22. If the president is elected by plurality, that usually means just two major parties. If the president is elected by a majority-runoff system, the number of major parties may be higher. See Shugart and Taagepera (1994). Of the concurrent elections systems analyzed here, only Ecuador uses the majority-runoff system.

23. Pluralitylike rules include the U.S. electoral college, given that the electors themselves are chosen by plurality rule (on a statewide basis in most states), and the Costa Rican provision that a runoff is held only in the event that no candidate obtains 40% of the vote.

24. Such a conclusion would be supported also by the example of Colombia, where elections are technically nonconcurrent (except for 1974) but are generally held at the point at which over 90% of the time between presidential elections has elapsed. Therefore, they are nearly concurrent, being held during the campaign for the next president. Their pattern resembles that of concurrent elections and localizing electoral

rules—an almost "pure" two-party system with occasional divided government.

25. Observations from Ecuador are excluded in these two statistical tests, as this is the only case for which concurrent elections and majority-runoff presidential elections are used in combination. As noted, the logic of the argument being tested applies to plurality rule for the presidency.

26. What is actually entered here is "effective" magnitude, which takes into account the effects of interdistrict variations in magnitude and of thresholds. See the Appendix.

27. Indeed, single-member districts are themselves localizing in the sense that they give each member a defined geographical constituency that he or she uniquely represents. However, the degree to which members can tailor their messages to their own districts varies according to how nationalizing other features of electoral and party rules are. Thus single-member districts in systems where the vote turns more on national concerns (e.g., in the United Kingdom and Canada), rather than on district characteristics (as in the United States) have a lower personal vote (Cain, Ferejohn, and Fiorina 1987) and also a greater number of parties (Epstein 1967; Shugart and Carey 1992, 226–29).

28. The coefficients (and standard errors) are as follows. With no dummy for the United States, the constant is .05 (.92), the coefficient on TIME is 1.14 (.55), and that on NUMPAR is −.52 (.25). With the U.S. dummy, the results are −.40 (.88), 1.20TIME (.56), −.39NUMPAR (.23), and .87USA (.52).

29. For reasons noted, these arguments about concurrent elections apply only when the president is elected by plurality or a similar electoral formula, not when majority runoff is used.

30. I have tried to avoid implying that the causal effects run in only one direction, from institutions to party organization to voter behavior. For instance, where, for whatever reasons, parties emerge as decentralized organizations, rules are likely to be adopted that weaken the nationalizing potential of elections. For present purposes, however, I am concerned with the proximate effect on voters' perceptions of whether or not their presidential and legislative votes have the same meaning with regard to national policy. Once chosen, rules affect behavior; such a statement by no means obviates the importance of analysis of the interests that lie behind the choice of rules.

31. In this vein, Palmer (1993) notes that unlike elections in Westminster parliamentary systems (and by extension in presidential systems that use nationalizing elections), which give voters policy options at election time, elections in what he terms the "congressional" U.S. system do little more than establish the identities of the players, who then "transact" with one another to produce policy.

32. The French constitution also provides for no presidential veto, thus obviating the need for coalitionlike government when the president faces an opposition majority. However, if (unlike France, but as in the United States and in Latin American presidentialism) the president retained control over the cabinet even in an opposition-majority situation *and* had no veto, it does not follow that governance would be smooth. Such a president remains chief executive, yet is practically assured of being marginalized in the legislative process, a situation that may have pernicious effects of its own, even tempting the president to use extraconstitutional measures to try to influence policy. For a fuller discussion of these issues and data on how often presidents have insufficient legislative support to be able to sustain vetoes, see Shugart and Mainwaring (n.d.)

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COMMITMENT, DEFERENCE, AND LEGISLATIVE INSTITUTIONS

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Informational theories of legislative institutions have demonstrated the potential collective gains from committee specialization due to the use of restrictive amendment procedures. For these benefits to be realized, however, the floor majority must overcome a commitment problem. I demonstrate that in a multistage game, majority rule, combined with the fact that generations of legislators overlap, allows the legislature to solve the commitment problem and to sustain legislative norms and institutions even if legislators are "finitely lived." The model suggests that rather than restrictive amendment procedures, it is the committee's expectation of floor deference that sustains committee specialization. As a consequence, legislative norms may collapse due to an unexpected influx of new members even if the distribution of preferences in the legislature does not change. Finally, I discuss consequences for a comparative study of legislative institutions.

The recent formal literature on legislative politics has emphasized the importance of institutions in legislative decision making (Gilligan and Krehbiel 1987; Krehbiel 1991; Shepsle and Weingast 1987; Weingast and Marshall 1988). Typically, legislative institutions such as committees have been characterized by procedural prerogatives (like gatekeeping, proposal, or veto powers) that are exogenous to the model. Institutions then correspond to particular game forms. Given those rules, actors play best-response strategies that result in "institutional equilibria" (Shepsle 1986). In real legislatures, however, a simple or qualified majority is typically able to change the institutional structure of the assembly.[1] But then the question arises why legislative institutions can be sustained, why they constitute "equilibrium institutions" (Riker 1980; Shepsle 1986).

The notion of institutions as equilibria presupposes the explicit specification of an underlying game in which legislative institutions emerge directly from individually rational behavior of legislators. There has been a small but growing literature on political institutions as equilibria;[2] yet legislative institutions have been all but ignored.[3] Researchers have, rather, focused on different variants of the prisoner's dilemma as a paradigm of social interaction (Calvert 1992, n.d.).

I shall show how particular features of legislative decision making can be utilized to sustain a committee system that is characterized by norms of deference to a proposing committee. I shall thereby focus on informational theories of legislative organizations (Cooper 1970; Gilligan and Krehbiel 1987; Krehbiel 1991). According to informational theories of legislative committees, committees are to be interpreted as agents of the floor that provide the collective benefit of reducing uncertainty about policy choices by learning about policy consequences and transmitting the obtained information to the floor. Acquiring such expertise, however, may be very costly especially in the case of complex legislation. So if floor and committee preferences do not coincide, floor majorities have to create incentives to committee members to incur the costs of specialization and credibly to transmit the acquired information, for example, by granting the committee restrictive amendment procedures that allow the committee to bias the final policy in its favor.

One of the main problems of these models is that typically procedural questions are decided by a floor majority after the committee will have already incurred the cost of providing these benefits. This, for instance, is true of the U.S. House where the Rules Committee grants a rule after the committee has already completed the markup process. Proposal by the Rules Committee, however are subject to majority approval and may be amended on the floor.[4] Therefore, the floor faces a commitment problem. Either specialization is sufficiently costly for the committee not to specialize in the absence of a closed rule, in which case the floor must be able to commit itself to a restrictive amendment procedure (an impossibility in a single-shot model) or specialization costs are low enough that the committee would specialize even under a open rule. Then there is no reason for the floor to grant a closed rule. The ability to grant a closed rule thus does not solve the commitment problem, since amendment rules are voted on after the committee has already drafted its proposal. Rather, a credible floor commitment to defer to the committee is necessary for the committee to specialize.

I suggest a model that solves the commitment problem while critically relying on a majoritarian decision rule. In fact, one of the main results of this analysis is to show how in equilibrium a legislative majority can credibly defer to a legislative committee, although no individual legislator, were he decisive, could. In particular, I suggest a multistage voting game with overlapping generations of "finitely lived" legislators. Overlapping-generations models have been employed in economics (Crémer 1986; Kreps 1991) and, more recently, in political science (Alesina and Spear 1987; Shepsle and Nalebuff 1990; Soskice, Bates, and Epstein 1992).

These models critically rely on the fact that an actor receives some special reward in the last period that would be lost if he failed to cooperate in a previous period. Tabellini (1987) as well as Brennan and Kliemt (1992) suggest cases in which majority rule could be used to sustain cooperation in overlapping generations models. In these models there is no need for a last-period reward to sustain cooperation. My model combines the insights from overlapping-generations models with a spatial voting model. In particular, I derive necessary and sufficient conditions for the existence of norms of deference and specialization. One of the unexpected consequences of the analysis is that norms of deference may collapse due to a large influx of new legislators even if the new legislators have exactly the same preferences as the senior members they replace.

I shall introduce a stage game that captures the incentives for commitment by the legislature and shows the impossibility of commitment in a single-shot game, then derive necessary and sufficient conditions for committee deference in a multistage game. The results imply that in equilibrium, norms of granting closed rules and norms of deference are equivalent. I shall provide comparative static results and discuss empirically testable implications of the model and its relevance to a comparative theory of legislative institutions. Proofs of the results are relegated to the Appendix.

THE STAGE GAME

The stage game is an $n + 1$ actor voting game with a committee k and n legislators with $n \geq 3$ odd. I denote generic actors by j ($j = 1, \ldots, n, k$), legislators by i ($i = 1, \ldots, n$). The actors interact in a sequence of decisions that result in a final policy $p \in \Re$. The consequences of a chosen policy are ex ante uncertain. Let $\omega \in \Re$ be a random variable with a continuous probability density function $f(\omega)$, $\bar{\omega}$ representing the mean and σ^2 the variance. We assume that $\bar{\omega}$ and σ^2 are finite and common knowledge. The consequences $x \in \Re$ of a policy p are represented by $x = p + \omega$ (Austen-Smith and Riker 1987). This formally captures the fundamental assumption that legislators are initially uncertain about the consequences of their actions. Actor j's preferences over consequences are represented by quadratic von Neumann–Morgenstern utility functions $u_j(.)$ with ideal point x_j. Thus I assume that the actors are risk-averse.[5] Consequently, they benefit from reducing uncertainty over policy consequences.

In the first stage, the committee decides whether it wants to acquire information about ω at a cost c ("specialize"). Thus the committee picks an action $s \in \{0, 1\}$, where 1 denotes the choice to specialize. If $s = 1$, ω becomes common knowledge. In contrast to the standard approach in informational theories, the mechanism used to transmit information about ω credibly to the floor is left unspecified. Previous research has suggested a variety of transmission mechanisms that may provide a foundation for my assumption: bill proposal (Gilligan and Krehbiel 1987, 1989), debate (Austen-Smith 1990), or hearings (Diermeier and Feddersen 1992). These mechanisms correspond to different types of signaling games.

Gilligan and Krehbiel (1987) demonstrate how even a committee of like-minded preference outliers can credibly reveal private information to the floor. The key idea is that if the committee can be sure that the floor will not amend its proposal before the final vote,[6] then proposing a bill corresponds to sending a costly signal. For certain parameter values a separating equilibrium exists, so that the privately held information is fully revealed to the floor. So one possible justification for our assumption of full information transmission (but not the only one) is to assume the existence of a unique separating equilibrium in a Gilligan/Krehbiel proposal game.[7] Let

$$I := \begin{cases} \Re & \text{if } s = 1 \\ \{\emptyset\} & \text{if } s = 0 \end{cases}$$

represent the public information available after the committee's decision whether to specialize. So if an actor observes a single real number, then he knows the realization of ω and, of course, that the committee has specialized. If he observes the empty set, then he knows that the committee has not specialized. This implies that the committee's decision to specialize or not is common knowledge. Elements of I are denoted by η. Now the committee reports a bill b to the floor. We represent this choice by $b{:}I \rightarrow \Re$. Then the floor decides whether to accept the committee's recommendation or to refer the bill to the floor for consideration under an open rule.

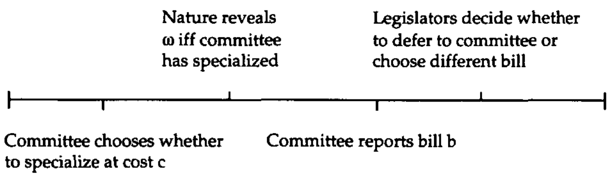
Again, I leave the procedural details unspecified. This framework captures, for example, the case where the floor chooses an amendment rule, closed or open, *after* the committee has submitted a bill for consideration,[8] or where a rules committee recommends a procedure and the floor has to decide whether to amend it. Finally, one can interpret the floor's action as a choice between deferring or overriding the committee under an open rule. This interpretation focuses on the norms of committee deference, rather than on the particular procedure used, and is especially relevant in studies of the U.S. Senate (Sinclair 1989; S. Smith 1989).

I represent a generic legislator's decision whether to defer to the committee as a function $v_i{:}\Re \times I \rightarrow \{0, 1\}$, where $v_i(b, \eta) = 1$ represents i's decision to defer to the committee and $v(b, \eta) =: (v_1(b, \eta), \ldots v_n(b, \eta))$. The consideration of pure strategies is without loss of generality. In the case of indifference I assume that $v_i(b, \eta) = 1$. The committee's proposal is accepted if a majority of legislators defers to the committee. There are no abstentions. Further, I assume that legislators are sequentially rational and never use weakly dominated strategies.[9] Note that for notational convenience I assume that the committee does not vote. We can always postulate the existence of a (voting) legislator with the same preferences as the committee.

If the committee's bill is accepted, then the game

FIGURE 1

Decision Sequence



ends and payoffs are assigned. In the case of rejection, every legislator makes a proposal. This determines some binary agenda. Then by Black's Theorem and Farquharson's Theorem the outcome of this voting game will be the policy that maximizes the median legislator's expected utility. Let x_m be the median's preferred outcome with $x_m = 0 < x_k$. So I assume that the committee is biased and normalize the payoffs such that x_k represents the distance between the committee and the floor median. Note that for $\eta = \omega$, we have $p = -\omega$, while $\eta = \varnothing$ implies $p = -\bar{\omega}$. Thus, every η induces a unique policy position p_m. Let $p_m(\eta)$ denote the median's preferred policy, given information η. So by sequential rationality i's decision whether to defer to the committee reduces to a choice between $b(\eta)$ and $p_m(\eta)$. In order to simplify notation I suppress the details of the agenda setting and voting game and focus on $v_i(.)$. Under the stated assumptions, this is without loss of generality. The structure of the stage game is summarized in Figure 1.

Then a *stage game strategy* for the committee is a pair $k := (s, b(.))$. For the legislators the stage game strategy is simply $v_i(. , .)$. Let $v := (v_1, \ldots, v_n)$. This allows me to define the payoffs $\pi_j(.)$ for the respective subgames induced by η.

$$\pi_j(v, b(\eta), \eta) :=$$

$$\begin{cases} -x_j^2 & \text{if } \eta \in \Re \text{ and } \sum_i v_i \leq (n-1)/2 \\ -(b+\omega-x_j)^2 & \text{if } \eta \in \Re \text{ and } \sum_i v_i > (n-1)/2 \\ -x_j^2 - \sigma^2 & \text{if } \eta = \varnothing \text{ and } \sum_i v_i \leq (n-1)/2 \\ -(b+\bar{\omega}-x_j)^2 - \sigma^2 & \text{if } \eta = \varnothing \text{ and } \sum_i v_i > (n-1)/2. \end{cases}$$

Then the payoffs for the stage game are

$$\pi_i(v, k) := \pi_i(v, b(\eta), \eta)$$

$$\pi_k(v, k) := \begin{cases} \pi_k(v, b(\eta), \eta) - c & \text{if } s = 1 \\ \pi_k(v, b(\eta), \eta) & \text{otherwise.} \end{cases}$$

DEFINITION 1. *An* n + *1-tuple* (k^*, v^*) *is an* equilibrium for the stage game *iff for all* $i = 1, \ldots, n$, *all* $\eta \in I$, $b(\eta)$, $v_i(b(\eta), \eta) \in \Re$, $s \in \{0, 1\}$,

i. $v_i^*(b(\eta), \eta) \in \text{argmax } \pi_i(v_i | v_{-i}^*, b(\eta), \eta)$
ii. $b^*(\eta) \in \text{argmax } \pi_k(b(\eta) | v^*, \eta)$
iii. $s^* \in \text{argmax } \pi_k(s | v^*, b^*)$.

PROPOSITION 1. *If* $\sigma^2 < c$, *then along the unique subgame-perfect equilibrium path of the stage game, the committee never specializes, and the chosen policy equals the median voters most preferred expected policy.*[10]

The intuition behind this result is that the committee anticipates that no matter what bill it proposes, the floor will not defer to the committee and rather will propose a bill that yields the pivotal voter's ideal point. But then there is no incentive for the committee to specialize if the costs to specialization are sufficiently high.

From now on, I assume that $x_k^2 + \sigma^2 \geq c > \sigma^2 \geq x_k^2$. While $\sigma^2 \geq x_k^2$ means that the floor median prefers the committee's ideal point to any outcome under uncertainty, assuming that $x_k^2 + \sigma^2 \geq c > \sigma^2$ implies that the committee's decision whether to specialize depends on its expectations about what will happen to its bill on the floor. This is the critical case for a theory of committee deference, since if specialization is too costly the committee would never specialize even if it would receive its ideal point ($c > x_k^2 + \sigma^2$). Yet if specialization costs are so low that the committee would specialize even if the median voter's ideal point prevails ($c \leq \sigma^2$), there is no need for the floor to defer to the committee, since it could enjoy all informational benefits without having to suffer distributional losses from a biased outcome.

Proposition 1 implies that the floor faces a commitment dilemma. The intuition is that the floor would like to commit itself to respect the committee's choice but is unable to do so, because in actual legislatures a floor majority can always roll a committee on the floor. But since this is anticipated by the committee, the committee never specializes and the actors are stuck in a Pareto-inefficient outcome. If procedural votes occur after the committee has specialized, closed rules cannot solve the commitment problem because the floor majority can never guarantee that it will actually vote for a closed rule when the committee has specialized. Rather, the floor has an incentive to adopt an open rule in order to receive its ideal point, and we are back in the commitment dilemma.

I shall present a multistage game where committee deference is generated endogenously. In fact, in the model no *individual* legislator could commit to deferring to the committee, while an assembly using majority rule can.

THE MULTISTAGE GAME

The general framework I consider is a multistage voting game with overlapping generations of "finitely lived" actors (Brennan and Kliemt 1992; Cremer 1986; Kandori 1992; Salant 1991; L. Smith 1992; Tabellini 1987). There are n teams of players with n finite. Each team has infinitely many members. All members of a team i have the same preferences. Every member of team i lives $T_i \in N$ periods and is then replaced by another member of the team.

One possibility to justify this assumption about team members is to interpret a legislator's preferences as being induced by the relevant constituency characteristics (e.g., districts or members of local or national party organizations). According to this interpretation, I assume that these characteristics are expected to be fairly stable. In fact, after characterizing the equilibrium for the multistage game I shall use comparative statics to analyze how the equilibrium responds to unexpected changes in constituency characteristics. Also, there is a team of committee members whose members live T_k periods. In every period the current players play a round of the stage game. The draws of ω are independent. Payoffs are assigned after every round and are discounted by a discount factor δ_i. Players try to maximize their individual discounted lifetime average payoff. The definition of the game is common knowledge.

The advantage of a model with "finitely lived" actors is that it allows me to demonstrate that collective properties (e.g., the decision rule used or the age distribution in the chamber) are critical in sustaining norms of deference and specialization, independently of the reputations of individual legislators.

In the model deference can be enforced by a simple trigger-style punishment that uses only limited information from previous periods. I shall show that for sustaining committee specialization in equilibrium it is sufficient that the actors condition their choice on the number of periods they will still participate in the game and on past deference decisions by the floor. For any agent j denote the number of periods j will play before he is replaced by the next member of the team by $\vartheta_j \in N$ with $\vartheta_j \leq T_j$. Let $\vartheta := (\vartheta_1, \ldots, \vartheta_n, \vartheta_k)$. Also let $z \in \{0, 1\}$. The state variable z is 0 if the floor has ever failed to defer to the committee or if the committee did not specialize in the current period, and 1 otherwise. Then a *state* y is defined as $y{:} = (z, \vartheta)$, with the following deterministic transition function $q(y'|y, (k, v))$ defined as

$$q((z', .)|(z, .), (k, v)) :=$$

$$\begin{cases} 1 & \text{if } z = 1,\ s = 1, \text{ and} \\ & \quad \sum_i v_i(b(\eta), \eta) > (n-1)/2 \\ 0 & \text{otherwise,} \end{cases}$$

with initial value $z_o = 1$, and for all j:

$$q((., \vartheta_j', \vartheta_{-j})|(., \vartheta), (k, v)) := \begin{cases} \vartheta_j - 1 & \text{if } \vartheta_j > 1 \\ T_j & \text{if } \vartheta_j = 1, \end{cases}$$

with some initial value $\vartheta \in N^n$. The set of states is denoted Y. Note that the transition function for ϑ is independent of (k, v), a fact that will be used later. Note that $\vartheta_i > 0$ always.

Payoffs are assigned after every period. Let A_j be the set of stage game strategies for actor i and let $A := A_k \times A_1 \times \ldots \times A_n$. A *stationary strategy* for k is a function $\kappa{:}Y \rightarrow A_k$, for i a function $\nu_i{:}Y \times I \rightarrow A_i$. Again let $\nu := (\nu_i, \ldots, \nu_n)$. Then using the definition of stage-game payoffs $\pi_j(.)$ I can recursively define the expected period t payoffs $\pi_j^t(.)$ for the multistage game as

$$\pi_j^1(y;\ \kappa, \nu) = \pi_j(k, b)$$

$$\pi_j^{t+1}(y;\ \kappa, \nu) = \sum_{y' \in Y} q(y'|y, \kappa(y), \nu(y))\pi_j^t(y;\ \kappa, \nu).$$

This then allows me to define the *discounted total expected payoff for the multistage game for* j *given state* y *and* (κ, ν) *as*

$$W_j(y;\ \kappa, \nu, \delta_j) = \sum_{t=1}^{\vartheta_j} \delta_j^{t-1}\pi_j^t(y;\ \kappa, \nu).$$

$W_j(y;\ \kappa, \nu, \delta_j)$ is always well defined, because ϑ_j is finite and r_j is bounded for all j. Since I consider only fixed δ_j, I typically suppress the last argument of $W_j(y;\ \kappa, \nu, \delta_j)$.

Definition 2. *A strategy combination* (κ^*, ν^*) *is an* equilibrium for the multistage game *if and only if for all* i = *1, . . . ,* n, *all* y ∈ Y, *all* ν_{i} *and* κ:

$$\mathrm{W_i}(\mathrm{y};\ \kappa^*, \nu_i^*, \nu_{-i}^*) \geq \mathrm{Wi}(\mathrm{y};\ \kappa^*, \nu_i, \nu_{-i}^*)$$

and

$$\mathrm{W_k}(\mathrm{y};\ \kappa^*, \nu^*) \geq \mathrm{W_k}(\mathrm{y};\ \kappa, \nu^*).$$

For (κ^*, ν^*), $W_j(y;\ \kappa^*, \nu^*)$ is called the *value function for* j *given* (κ^*, ν^*). By theorem 3 of Sobel 1971, (κ^*, ν^*) is a Nash equilibrium in stationary strategies if and only if for all $i = 1, \ldots, n$ and for all y

$$(^*)W_k(y;\ \kappa^*, \nu^*) = \max_k \{\pi_k(k, v^*) + \delta_k W_k(y';\ \kappa^*, \nu^*)\}$$

$$(^{**})W_i(y;\ \kappa^*, \nu^*) = \max_{v_i} \{\pi_i(k^*, v_i, v_{-i}^*) + \delta_i W_i(y';\ \kappa^*, \nu_i^*, \nu_{-i}^*)\},$$

where $y' = q(y'|y, (k, v))$.

Note that given that $q(.|.)$ is deterministic, y' is well defined. Blackwell's Lemma guarantees that Nash equilibria in stationary strategies remain Nash equilibria even if the class of admissible strategies is extended to include nonstationary strategies. So in order to check whether (κ, ν) is an equilibrium, we only have to show that strategies in (κ, ν) are mutual best responses in a game defined by a collection of associated value functions satisfying the Bellman equations (*) and (**). Actors are still required never to use weakly dominated strategies and attention is restricted to subgame-perfect equilibria.

I now proceed to show the existence of deference equilibria. The underlying intuition is straightforward. Assume that a legislator is pivotal. Then in a legislator's last period he will only choose an action that maximizes his one-period payoff. In earlier periods, however, deference to the committee can be sustained by the use of a trigger-type punishment. So a committee will specialize unless the floor majority ever fails to defer to a specialized committee. In the latter case, the committee will permanently switch to its one-period best response (no specialization); if the committee fails to specialize, then the floor will play his single-stage best response forever (i.e., not defer to the committee). Then under certain conditions, deference to the committee may be a best response in the multistage game, although it is not myopically optimal.

In order to characterize these equilibria, I analyze the incentives of a pivotal legislator whether to defer to a committee that uses the specified trigger-style strategy. For a fixed δ_i, x_i, and x_k let

$$\gamma_i(\vartheta) := (\delta_i - \delta_i^{\vartheta_i})\sigma^2 - (1 - \delta_i^{\vartheta_i})x_k(x_k - 2x_i).$$

Note that $x_k(x_k - 2x_i)$ is equal to $u_i(x_k) - u_i(x_m)$. The term $(1 - \delta_i^{\vartheta_i})x_k(x_k - 2x_i)$ can thus be interpreted as legislator i's average expected loss due to deferring to a specialized committee instead of voting for the median legislator's position provided the committee specializes for the remaining periods that i participates in the game. In contrast, $(\delta_i - \delta_i^{\vartheta_i})\sigma^2$ captures i's average expected loss due to uncertainty about policy consequences under the assumption that the median legislator's ideal point is chosen as the floor's policy. So $\gamma_i(\vartheta)$ reflects the net benefits of a generic legislator gains by deferring to an informed yet biased committee, instead of voting for the median legislator with the consequence of no future specialization by the committee.

Note that $(\delta_i - \delta_i^{\vartheta_i})\sigma^2 = \delta_i(1 - \delta_i^{\vartheta_i - 1})\sigma^2$. This reflects the fact that at the time t when the legislator has to decide whether to defer to the committee, the committee has already specialized for this period. So potential informational losses can only occur in periods $t' \geq t + 1$ and are thus discounted accordingly, while distributional losses due to deferring to the committee are also incurred in period t. This implies that for $\vartheta_i = 1$, $(\delta_i - \delta_i^{\vartheta_i})\sigma^2 = 0$. So if i is in the last period, $\gamma_i(\vartheta) = -(1 - \delta_i)x_k(x_k - 2x_i)$, which implies that only distributional considerations are relevant. We can thus interpret $\gamma_i(\vartheta)$ as representing the net benefits from deferring to a specialized yet biased committee instead of voting for the median's position, but incurring future informational losses.

So far I have focused on the pivotal legislator and ignored the incentives of the committee to specialize. The committee's decision to specialize depends on two conditions: (1) the floor must have deferred to the committee previously and (2) the next periods' pivotal legislators must have an incentive to defer to the committee. Otherwise, the committee will anticipate that in the next period the floor will not defer to the committee even if it specialized, which would lead to the committee's worst outcome. The committee specializes if and only if the floor's deference is credible.[11] In order to capture this intuition formally for every ϑ, one can calculate the median $\gamma_i(\vartheta)$ value. So, let

$$\mu(\vartheta) := \text{median:}\{\gamma \in \Re \mid \gamma = \gamma_i(\vartheta) \text{ for some } i = 1, \ldots, n\}.$$

This allows me to specify the candidate for a deference equilibrium.

DEFINITION 3. *For fixed* δ_j, x_j, σ^2 majority deference *is a strategy combination* (κ, ν) *such that for all* $y \in Y$, $i = 1, \ldots, n$

$$\kappa(y) = \begin{cases} (1, x_k - \omega) & \textit{if } y = (1, \vartheta) \\ (0, x_k - \bar{\omega}) & \textit{otherwise} \end{cases}$$

for all i *such that* $x_k(x_k - 2x_i) \leq 0$

$$\nu_i(y, \eta) = 1$$

for all i *such that* $x_k(x_k - 2x_i) > 0$

$$\nu_i(y, \eta) = \begin{cases} 1 & \textit{if } y = (1, \vartheta),\ \gamma_i(\vartheta) \geq 0 \textit{ and } \eta \in \Re \\ 0 & \textit{otherwise} \end{cases}$$

Note that if i is in the last period (i.e., $\vartheta_i = 1$), then the condition on $\nu_i(y, \eta)$ reduces to $0 \geq x_k(x_k - 2x_i)$. But $0 \geq x_k(x_k - 2x_i)$ if and only if $u_i(x_m) \leq u_i(x_k)$. So in this case, i will play 1 only if this action would be optimal in the one-stage game. Moreover, legislators who prefer the committee's ideal point to the floor median's ideal point will always defer to the committee.

This allows us a simple characterization of deference equilibria. At every time t, the losses for the associated legislator with median expected utility due to σ^2 have to be higher than the distributional losses due to deferring to the committee. For a fixed ϑ, $\mu(\vartheta)$ is the median legislator in net benefits, provided the players use deference strategies subsequently. So given majority rule, one can sustain deference as an equilibrium provided that no $\mu(\vartheta)$ has an incentive to defect. But for this condition to hold, it suffices to consider the median in expected utility with the highest incentive to defect.

One can formally capture this intuition by recursively defining the following sequence. For a given ϑ let

$$\vartheta^1 = \vartheta$$

$$\vartheta^{t+1} = q(\vartheta' \mid \vartheta^t, (k, v)) \text{ for some } (k, v).$$

Note that since with respect to ϑ, $q(.|.)$ is independent of (k, v), this is well defined. Then let $\Theta(\vartheta) := \langle \vartheta^t \rangle_{t=1}^{\infty}$. The intended interpretation for $\Theta(\vartheta)$ is a sequence of age distributions generated by an initial distribution ϑ and the fixed transition rule $q(.|.)$. Since $q(.|.)$ is deterministic, $\Theta(\vartheta)$ is uniquely determined by a given ϑ. Hence, if a legislator knows ϑ and $q(.|.)$, she also knows $\Theta(\vartheta)$. Now let

$$\underline{\mu}(\Theta(\vartheta)) := \min\{\gamma \in \Re \mid \gamma = \mu(\vartheta) \text{ for some } \vartheta \in \Theta(\vartheta)\}.$$

So if for the median legislator with the lowest expected net benefit, informational losses are always higher than distributional losses (i.e., if $\underline{\mu}(\Theta(\vartheta)) \geq 0$), then floor deference can be sustained in equilibrium provided the committee has an incentive to specialize.

In the Appendix I prove the following theorem capturing this intuition.

THEOREM 1. *Let* $c > \sigma^2$. *Then majority deference is a subgame-perfect equilibrium for an initial distribution* ϑ *if and only if* $\underline{\mu}(\Theta(\vartheta)) \geq 0$.

DISCUSSION OF THE MODEL

First it should be noted that the legislator with the median net benefit is not necessarily identical with the median voter but, rather, typically changes over time, depending on the sequence of age distributions at time t and on the list of ideal points x_i and x_k. Note that the condition on $\underline{\mu}(\Theta(\vartheta))$ is both necessary and sufficient. Note that the result holds independently of T_k. So if for some period the median with the lowest net benefits would defect, then legislators and the committee will anticipate this and the suggested equilibrium unravels by backward induction. Even a committee that lives only for one period will specialize if it expects the floor to defer to its proposal.

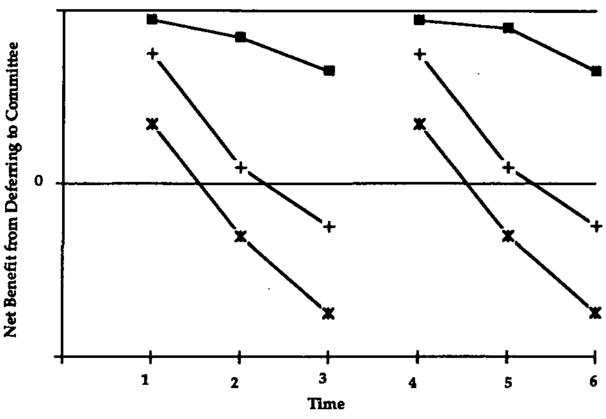
The intuition underlying the theorem can be illustrated by the following example. Consider a legislature with three teams of voting members, denoted *, +, and ■. Suppose each player of a particular team plays for three periods. In Figure 2, the expected net benefits from deferring to a biased, yet permanently specializing committee for two members of each team are calculated.

The gaps in the lines indicate that after three periods every member is replaced by a new member of the same team. In this particular example, the members of team ■ would always defer to the committee, * only in period 1, and so on. The initial age structure is such that all members of all teams enter and leave the legislature at the same time. So there is no generational overlap. Now consider period 3. In this period there is a majority for amending the committee's proposal. This will be anticipated by the committee in period 3, and the committee will not specialize; but then there is no incentive for the legislators to defer to the committee in period 2, since they anticipate that there will be no gains from specialization in period 3, and the suggested equilibrium unravels by backward induction. So in order for deference to be an equilibrium, a majority of legislators must prefer to defer to a specialized committee. Figure 3, a sequence of age distributions, is an example.

As Figure 2 and 3 indicate, generational overlap is a necessary condition for deference being an equilibrium. However, it is not sufficient as Figure 4 shows. Although generations overlap, the particular sequence of age distributions does not allow to sustain majority deference. In period 2, a majority would choose to amend the committee's proposal. But given that the actors are sequentially rational, this invokes the usual backward induction argument. This example indicates that both the distribution of ideal points and the sequence of age distribution is critical to whether committee specialization can be sustained in equilibrium. Note that majority rule is critical in solving the commitment problem. Under unanimity, deference would be impossible.

FIGURE 2

Age-Preference Configuration for a Three-Member Legislature

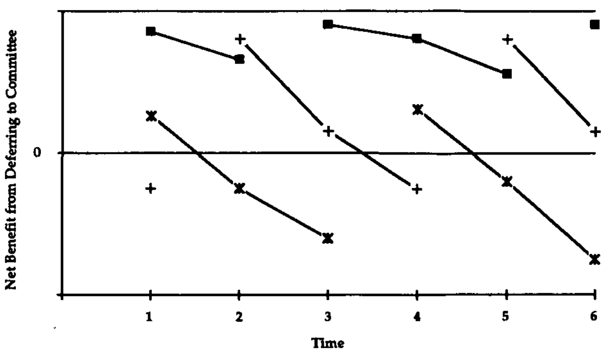


Note: +, *, and ■ represent different teams of legislators characterized by different ideal points on policy outcomes. An individual member is symbolized by three such points connected by a line. The gaps indicate that after three periods every member is replaced by a new member of the team. The net benefits refer to the expected payoff of deferring to a permanently specializing, yet biased committee instead of voting for the median's position, but incurring future informational losses.

The example also suggests a new explanation for the collapse of norms in legislatures. Consider the age distribution in Figure 3 at time $t = 3$. Here the pivotal actor is +. Now suppose that due to an unexpected event (e.g., a scandal like the House check writing affair), at $t = 3$, + is replaced by a new member with precisely the same preferences on outcomes. Note that this corresponds to a situation like that of Figure 4, where norms of deference can no longer be sustained. This collapse of norms will occur, although in the critical period $t = 3$, the pivotal legislator's expected utility is higher than in the case of deference (Figure 3), and the distribution of preferences on outcomes remains fixed. A shock to the

FIGURE 3

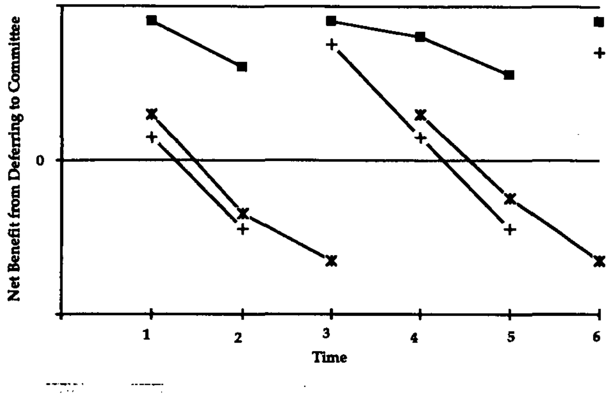
Age-Preference Configuration with Deference



Note: Here legislators enter and exit at different times. At $t = 1$, for instance, + is in the last period, while * is in the first. Given this overlap structure, there now is a majority with a net benefit from deferring to the committee at every period. Norms of deference can be sustained in equilibrium.

FIGURE 4

Age-Preference Configuration with No Deference



Note: This is a case where despite generational overlap, norms of deference cannot be sustained. In period 2 a majority will not defer to the committee. The committee will anticipate this, and the suggested norm unravels by backward induction.

age distribution is enough to bring about a collapse of legislative norms.[12]

It is useful to compare my model with a standard repeated game with "infinitely lived" agents.[13] If ϑ_1 increases, then $\gamma_i(\vartheta)$ weakly increases. So Theorem 1 implies that the longer individual legislators live, the more easily committee specialization can be sustained. In the limit, the median legislator in net benefits is constant over time. Moreover, if one assumes a common discount factor δ, then the median legislator equals the floor median in policy space, and the condition from Theorem 1 reduces to a familiar requirement on δ; that is, for deference to be an equilibrium, one needs $\delta \geq x_k^2/\sigma^2$.

Whether deference can be sustained in equilibrium depends only on the normalized distance between committee and floor median and on the uncertainty about policy consequences captured by σ^2. Both the exact distribution of legislators' ideal points and the sequence of age distributions (which in the finite case determine the legislator in net benefits for a particular period) are irrelevant under an infinite horizon.

It is worth noting that my simple punishment scheme does not assume that voting decisions by individual legislators are observable. Only the result of the procedural vote whether to defer to the committee must be known. So my theorem also holds for legislatures like the Italian parliament, where secret ballots are typically used in both procedural and final votes (D'Onofrio 1979).

So far, I have demonstrated the existence of a particular equilibrium but have not completely characterized the set of subgame-perfect stationary equilibria. Of particular interest is the case where $\underline{\mu}(\Theta(\vartheta)) < 0$. Here, in some period a majority would choose to amend a specialized committee's proposal. In order to investigate this question, the framework used has to be slightly modified.[14] In particular, the proposal strategy of the committee has to be modeled explicitly. Then the incentives of a pivotal legislator whether to defer to a committee depend on the particular committee proposal x. This implies that a legislator's net benefit from deferring to a specialized, yet biased, committee instead of voting for the median's position is now a function of x; that is, $\gamma_i(\vartheta, x)$ and, consequently, $\mu(\vartheta, x)$ and $\underline{\mu}(\Theta(\vartheta), x)$. Now consider the case where $\underline{\mu}(\Theta(\vartheta), x_k) < 0$. Theorem 1 implies that in this case, proposing x_k will not be a equilibrium strategy. But for some $x < x_k$ it might be that $\underline{\mu}(\Theta(\vartheta), x) \geq 0$. So for a moderate proposal, the floor may prefer to defer to the committee. As long as specialization costs are small enough, the committee might still benefit from specializing by proposing a moderately biased policy.

Concerning the cost of specialization, it was assumed that it is worthwhile for the committee to specialize if it receives its ideal point but that the committee will not specialize if the median legislator's ideal point is implemented. So for a given cost c there exists a policy $\underline{x}(c)$ such that the committee is indifferent between specializing with policy $\underline{x}(c)$ and not specializing with policy x_m, the median's preferred policy. So as long as it is worthwhile for the committee to bias the chosen policy in its favor while incurring the cost of specialization, equilibria are possible where the committee specializes and the floor defers to the committee. The committee, of course, will try to maximize the policy bias. Thus, in equilibrium the median legislator with the minimal expected net benefits, $\underline{\mu}(\Theta(\vartheta), x)$, will be exactly indifferent between accepting or amending the committee's proposal.

However, if $\underline{\mu}(\Theta(\vartheta), x) < 0$ for any policy closer to the committee's ideal point than $\underline{x}(c)$, then the distributional benefits to the committee are too small to compensate for specialization costs and the committee will never specialize.

IMPLICATIONS FOR A COMPARATIVE STUDY OF LEGISLATIVE INSTITUTIONS

Informational Rationale for Committees. Cooper (1970) argues that the main reason for the development of a strong committee system in the U.S. House of Representatives was the need to acquire expert information about policy consequences. In my model, the expected losses due to uncertainty are captured by σ^2. But $\gamma_i(\vartheta)$ is increasing in σ^2. Therefore, if uncertainty about policy consequences rises then, ceteris paribus, deference can be sustained more easily. Note, however, that the need for information is not sufficient for the establishment of committee deference. As indicated in Figure 4, in some age/preference distributions committee specialization cannot be sustained in equilibrium. In fact, strong committees in the House developed after the expected number of terms served increased significantly in the nineteenth century. As the model points out, longer terms facilitate overlap and thus the development of norms of deference.[15]

Restrictive Amendment Procedures. Gilligan and Krehbiel (1987) use Cooper's insight to explain the increased use of restrictive amendment procedures in the nineteenth century. Since restrictive amendment procedures allow a committee to bias the final policy, they provide the incentive to acquire costly information about policy consequences. Gilligan and Krehbiel (1989), however, point out that this argument critically depends on the possibility of procedural commitment.

The model suggests how this commitment problem can be solved. The solution, however, also suggests that it is not primarily the restrictive amendment procedure that sustains committee specialization but the committee's expectation that the floor will not amend its proposal. Closed rules are neither necessary nor sufficient to solve the commitment problem. Then what is the importance of explicit procedural rules? Kreps (1991) argues that clear rules facilitate cooperative outcomes since they reduce "misunderstandings" of whether a norm was violated or not. According to this line of reasoning, explicit rules help to overcome coordination problems, but cooperative outcomes can be sustained even in their absence.

Folkways of the Senate. The model shifts the attention from the presence of restrictive procedures to patterns of deference, which allows an application of the informational framework to legislative bodies where implicit norms regulate behavior, such as the U.S. Senate (Matthews 1960; Sinclair 1989; and S. Smith 1989). In the present context the apprenticeship norm, the norm of specialization, and the norm of committee deference are most relevant. In the model presented, these norms correspond to the committee strategy of specialization and the legislators' strategy of deferring to the committee. Our analysis suggests that these norms are mutually enforcing. The specialization of committee members makes it worthwhile for the floor to accept the committee's proposal. The expectation of distributional benefits makes it worthwhile for the committee to incur the costs of acquiring expertise.

Future research should investigate whether there is a monotonic relationship between the inclusiveness of decision rules and specialization, that is, whether the higher the proportion of members needed to pass a proposal, the more difficult it is to sustain a norm of committee deference. Such a result may then be used to explain differences in specialization between the House and Senate.[16]

Decline of Senate Folkways. Any convincing explanation of how norms can be sustained in equilibrium also has to specify conditions under which a norm is expected to collapse. Sinclair (1989), for instance, argues that an influx of liberal senators into a rather conservative Senate after the 1958 election resulted in the rapid decline of the norms concerning apprenticeship, specialization, and committee deference.[17] These unexpected changes can be captured by a comparative statics analysis that investigates the changes in equilibrium if ϑ, x_i, or δ_i are changed. Theorem 1 implies that adding more "extremist" (i.e., strongly opposed to the committee) or electorally insecure legislators will decrease some $\mu(\vartheta)$. Note that not all changes in ϑ, x_i, or δ_i will lead to a collapse of the floor deference. As long as $\mu(\Theta(\vartheta)) \geq 0$, deference will still remain an equilibrium. However, a radical change in the membership that leads to $\mu(\Theta(\vartheta)) < 0$ will lead to the collapse of the norm of committee deference. This breakdown will be immediate, since $\mu(\Theta(\vartheta)) < 0$ induces backward induction, which leads to a noncooperative outcome similar to Proposition 1. Moreover, since norms of specialization and deference sustain each other, the model suggests that these norms should collapse simultaneously. Theorem 1, however, also implies that a large unexpected change in membership alone may be sufficient for the collapse of norms even if those new members have the same preferences as those they replace. The model also suggests why there was no similar dramatic change in the House. Due to smaller turnover rates and the larger size of the House the resulting turnovers were simply not big enough to tip the balance.

Term Limits. The model also has some implications for the debate on term limits. It suggests that even in the presence of strict term limits, norms and institutions can be sustained. The assumption of potentially "infinitely lived" legislators (i.e., legislators that always have a positive probability to be reelected to the chamber) is not necessary. Of course, the particular design of term limits might matter, since it typically influences the degree of overlap between different generations. The folk theorems for overlapping generations models suggest that a longer overlap period facilitates cooperation (L. Smith 1992; Kandori 1992). In our model, this would be reflected in changes of $\mu(\vartheta)$.

Party Organizations. While the model presented is compatible with a model of committee reciprocity, it does not assume that there is more than one committee. This allows me to apply the model to the institutional development of collective bodies that strictly speaking do not legislate but whose decision processes are similar. Consider the case of legislative parties where members defer to a better informed steering committee[18] in deciding on a policy proposal. A related example concerns leadership elections in Western European parties. Typically, a central party committee recommends a candidate and the party members defer to the committee. If there are sufficiently many potential candidates and the consequences of electing a candidate are ex ante unknown, then the model presented here can be used to analyze patterns of deference to party committees.[19]

As a thought experiment, consider the case where party members wish to avoid the emergence of a party hierarchies in the legislature (i.e., a strong party steering committee to which back-benchers defer). In this case, the model recommends that they should, for instance, adopt a party constitution with term limits (all members of a party caucus entering and exiting the chamber at the same time) or require unanimous decisions on party proposals. This is precisely the strategy the German Greens adopted in order to prevent party hierarchies. The model, however, also indicates the costs of these decisions, namely, that informational benefits will be lost.

I have presented a simple model of legislative decision making that allows a legislature to sustain committee specialization and deference as equilibrium phenomena. The results of the analysis indicate a behavioral foundation for informational theories of legislative organization and link some of the empirical findings in legislative research.

Variants of the model should incorporate the richer structure of actual legislatures, especially parties and coalitions. This could help to resolve the question whether party discipline is inversely related to committee strength.

Another expansion of the model should relax the assumption of deterministic transition rules and develop a stochastic game where the probability to be reelected to the chamber is a function of previous actions. Such a model could indicate what restrictions have to be placed on the transition probability in order still to sustain committee specialization.

APPENDIX

Proposition 1. *If* $\sigma^2 < \mathrm{c}$, *then the unique subgame-perfect equilibrium path for the stage game is (i)* $\mathrm{s}^* = 0$, *(ii)* $\mathrm{b}^*(\bar{\omega}, \sigma^2) = \mathrm{x_k} - \bar{\omega}$, *(iii)* $\mathrm{v_i^*}(\mathrm{b}(\eta), \eta) = 0$ *for all* i *such that* $\mathrm{x_i^2} \leq (\mathrm{x_k} - \mathrm{x_i})^2$ *and* $\mathrm{v_i^*}(\mathrm{b}(\eta), \eta) = 1$ *for all* i *such that* $\mathrm{x_i^2} > (\mathrm{x_k} - \mathrm{x_i})^2$, *(iv)* $\mathrm{p}^* = -\omega$ *with* $\pi_\mathrm{j}(\mathrm{k}^*, \mathrm{v}^*) = -\mathrm{x_j^2} - \sigma^2$.

Proof of Proposition. The proof is straightforward by backward induction. If $\eta = \omega$, then by Black's Theorem and Farquharson's Theorem $p^* = -\omega$. Now if i were pivotal,

$$\pi_i(v_i = 0 | v^*_{-i}, b(\eta), \eta) = -x_i^2$$

$$\pi_i(v_i = 1 | v^*_{-i}, b(\eta), \eta) = -(b + \omega - x_i)^2.$$

Then $\Sigma_i\, v_i > (n - 1)/2$ if and only if $b + \omega = 0$. But since weakly dominated strategies are excluded, $b^* = x_k - \omega$ and $p^* = -\omega$. *If* $\eta \in \Re$, *then* $p^* = -\bar{\omega}$. Now if i were pivotal,

$$\pi_i(v_i = 0 | v^*_{-i}, b(\eta), \eta) = -x_i^2 - \sigma^2$$

$$\pi_i(v_i = 1 | v^*_{-i}, b(\eta), \eta) = -(b + \bar{\omega} - x_i)^2 - \sigma^2.$$

Then $\Sigma_i\, v_i > (n - 1)/2$ if and only if $b + \bar{\omega} = 0$. Therefore $b^* = x_k - \omega$ and $p^* = -\bar{\omega}$. Thus,

$$\pi_k(s = 0 | v^*, b^*) = -x_k^2 - \sigma^2 > x_k^2 - c = \pi_k(s = 1 | v^*, b^*)$$

Theorem 1. *Let* c $> \sigma^2$. *Then majority deference is a subgame-perfect equilibrium for an initial distribution* ϑ *if and only if* $\underline{\mu}(\Theta(\vartheta)) \geq 0$.

Proof of Theorem 1. To establish sufficiency, let $\underline{\mu}(\Theta(\vartheta)) \geq 0$. First, I define the value functions given (κ^*, ν^*), the suggested equilibrium. Note that for all $\vartheta_j \leq T_j$, $\vartheta_j = \vartheta_j' + 1[\mathrm{mod}T_j]$.

For $y = (0, \vartheta)$,

$$W_j(y; \kappa^*, \nu^*) = -\sum_{t=1}^{\vartheta_j[\mathrm{mod}\ T_j]} \delta_i^{t-1}(x_j^2 + \sigma^2)$$

$$= -\frac{1 - \delta_j^{\vartheta_j[\mathrm{mod}\ T_j]}}{1 - \delta_j}(x_j^2 + \sigma^2).$$

For $y = (1, \vartheta)$,

$$W_i(y; \kappa^*, \nu^*) = -\sum_{t=1}^{\vartheta_i[\mathrm{mod}\ T_i]} \delta_i^{t-1}(x_k - x_i)^2$$

$$= -\frac{1 - \delta_i^{\vartheta_i[\mathrm{mod}\ T_i]}}{1 - \delta_i}(x_k - x_i)^2.$$

For $i = 1, \ldots, n$,

$$W_k(y; \kappa^*, \nu^*) = -\sum_{t=1}^{\vartheta_j[\mathrm{mod}\ T_j]} \delta_i^{t-1} c = -\frac{1 - \delta_j^{\vartheta_j[\mathrm{mod}\ T_j]}}{1 - \delta_j}c,$$

First I show that the value functions satisfy the Bellman equations.
Let $y = (0, \vartheta)$. Then

$$W_j(y; \kappa^*, \nu^*) = -\frac{1 - \delta_j^{\vartheta_j}}{1 - \delta_j}(x_j^2 + \sigma^2)$$

$$= \frac{1 - \delta_j^{\vartheta_j'+1[\mathrm{mod}\ T_i]}}{1 - \delta_j}(x_j^2 + \sigma^2).$$

Now given $y = (0, \vartheta)$, $\kappa^*(y) = (0, x_k - \bar{\omega})$ and $\nu_i^*(y, \eta) = 0$ if $0 \geq x_k(x_k - 2x_i)$. For the median legislator $i = m$ this reduces to $0 \geq x_k^2$. Thus, $\Sigma_i\, v_i^* \leq (n - 1)/2$ and $z = 0$. Then $p = -\bar{\omega}$ and $y' = (0, \vartheta_j')$. Therefore $\pi_j(\kappa^*(y), \nu^*(y, \eta)) = -(x_j^2 + \sigma^2)$ and

$$W_j(y'; \kappa^*, \nu^*) = -\frac{1 - \delta_j^{\vartheta_j'[\mathrm{mod}\ T_j]}}{1 - \delta_j}(x_j^2 + \sigma^2).$$

But then

$$\pi_j(\kappa^*(y), \nu^*(y, \eta) + \delta_j W_j(y'; \kappa^*, \nu^*))$$

$$= -\frac{1 - \delta_j^{\vartheta_j'+1[\mathrm{mod}\ T_j]}}{1 - \delta_j}(x_j^2 + \sigma^2) = W_j(y; \kappa^*, \nu^*).$$

Let $y = (1, \vartheta)$. Then

$$W_i(y; \kappa^*, \nu^*) = -\frac{1 - \delta_i^{\vartheta_i[\mathrm{mod}\ T_i]}}{1 - \delta_i}(x_k + x_i)^2 \text{ and}$$

$$W_k(y;\, \kappa^*,\, \nu^*) = -\frac{1-\delta_k^{\vartheta_k[\mathrm{mod}\ T_i]}}{1-\delta_k}\, c.$$

Now assuming that i were pivotal $\nu_i(y) = 1$ if

$$(\delta_i - \delta_i^{\vartheta_i})\sigma^2 \geq (1-\delta_i^{\vartheta_i})x_k(x_k - 2x_i).$$

Since by assumption $\underline{\mu}(\Theta(\vartheta)) \geq 0$, $\Sigma_i\, v_i^* > (n-1)/2$ and $z = 1$. But then $p = x_k - \omega$ and $y' = (0,\, \vartheta_j - 1,\, \vartheta'_{-j})$. Hence $\pi_i(\kappa^*(y),\, \nu^*(y,\, \eta)) = -x_k(x_k - 2x_i)$ and $\pi_k(\kappa^*(y),\, \nu^*(y,\, \eta)) = -c$, as well as

$$W_i(y';\, \kappa^*,\, \nu^*) = -\frac{1-\delta_i^{\vartheta_i'[\mathrm{mod}\ T_i]}}{1-\delta_i}\,(x_k - x_i)^2 \text{ and}$$

$$W_k(y;\, \kappa^*,\, \nu^*) = -\frac{1-\delta_k^{\vartheta_k'[\mathrm{mod}\ T_k]}}{1-\delta_k}\, c.$$

But then

$$\pi_i(\kappa^*(y),\, \nu^*(y,\, \eta) + \delta_i W_i(y';\, \kappa^*,\, \nu^*))$$
$$= -\frac{1-\delta_i^{\vartheta_i'+1[\mathrm{mod}\ T_i]}}{1-\delta_i}\,(x_k^2 - x_i^2) = W_i(y;\, \kappa,\, \nu)$$

and

$$\pi_k(\kappa^*(y),\, \nu^*(y,\, \eta) + \delta_k W_k(y';\, \kappa^*,\, \nu^*))$$
$$= -\frac{1-\delta_k^{\vartheta_k'+1[\mathrm{mod}\ T_k]}}{1-\delta_k}\, c = W_k(y;\, \kappa,\, \nu).$$

Next I show that for all y, the strategies $\nu_1^*(.), \ldots, \nu_n(.)^*$ satisfy

$$W_i(y';\, \kappa^*,\, \nu^*) = \max_{v_i} \{\pi_i(\kappa^*(v_i,\, v_{-i}^*)$$
$$+ \delta_i W_i(y';\, \kappa^*,\, \nu_i^*,\, \nu_{-i}^*)\}$$

with $y' = q(y'|y,\, (k,\, v))$.

Assume i were pivotal. First note that for all i such that $x_k(x_k - 2x_i) \leq 0$, $\nu_i(y,\, \eta) = 1$ is weakly dominant. So I can restrict attention to the case where $x_k(x_k - 2x_i) > 0$. Let $y = (0,\, \vartheta)$ or $y = (1,\, \vartheta)$ with $\eta = \varnothing$. Then $y' = (0,\, \vartheta')$ and

$$W_i(y;\, \kappa^*,\, \nu_i(y,\, \eta) = 0,\, \nu_{-i}^*) = -(x_i^2 + \sigma^2)$$
$$- \delta_i \frac{1-\delta_j^{\vartheta_j'[\mathrm{mod}\ T_j]}}{1-\delta_j}\,(x_j^2 + \sigma^2)$$

while

$$W_i(y;\, \kappa^*,\, \nu_i(y,\, \eta) = 1,\, \nu_{-i}^*) = -[(x_k + x_i)^2 + \sigma^2]$$
$$- \delta_i \frac{1-\delta_j^{\vartheta_j'[\mathrm{mod}\ T_j]}}{1-\delta_j}\,(x_j^2 + \sigma^2).$$

But for $i = m$, this reduces to

$$W_i(y;\, \kappa^*,\, \nu_i(y,\, \eta) = 1,\, \nu_{-i}^*) \geq W_i(y;\, \kappa^*,\, \nu_i(y,\, \eta) = 0,$$
$$\nu_{-i}^*) \text{ iff } 0 \neq x_k^2.$$

So $\nu_i(y,\, \eta) = 0$ is optimal, and $\Sigma_i\, v_i^* \leq (n-1)/2$. Now suppose that $y = (1,\, \vartheta)$ with $\eta \in \mathfrak{R}$. Given that i is assumed to be pivotal, $\nu_i(y,\, \eta) = 0$ implies $y' = (0,\, \vartheta)$, as well as $\pi_i(\kappa^*(y),\, \nu^*(y,\, \eta)) = -x_i^2$ and

$$W_i(y';\, \kappa^*,\, \nu_i = 0,\, \nu_{-i}^*) = -\frac{1-\delta_i^{\vartheta_i'[\mathrm{mod}\ T_i]}}{1-\delta_i}\,(x_i^2 + \sigma^2).$$

If $\nu_i(y,\, \eta) = 1$, then $y' = (1,\, \vartheta')$. So, $\pi_i(\kappa^*(y),\, \nu^*(y,\, \eta)) = -(x_i - x_k)^2$ and

$$W_i(y';\, \kappa^*,\, \nu_i = 1,\, \nu_{-i}^*) = -\frac{1-\delta_i^{\vartheta_i'[\mathrm{mod}\ T_i]}}{1-\delta_i}\,(x_i - x_k)^2.$$

So $\nu_i(y,\, \eta) = 1$ is a best response if

$$-\frac{1-\delta_i^{\vartheta_i'+1[\mathrm{mod}\ T_i]}}{1-\delta_i}\,(x_i - x_k)^2 \geq -(x_i)^2$$
$$- \delta_i \frac{1-\delta_i^{\vartheta_i'[\mathrm{mod}\ T_i]}}{1-\delta_i}\,(x_i^2 + \sigma^2)$$

and

$$(\delta_i - \delta_i^{\vartheta_i})\sigma^2 \geq (1-\delta_i^{\vartheta_i})x_k(x_k - 2x_i).$$

Next I show that for all y the stationary strategy $\kappa^*(.)$ satisfies

$$W_k(y;\, \kappa^*,\, \nu^*) = \max_{k(y)} \{\pi_k(k,\, v^*) + \delta_k W_k(y';\, \kappa^*,\, \nu^*)\}.$$

Let $y = (0,\, \vartheta)$. Then $y' = (0,\, \vartheta')$. Thus

$$W_k(y;\, \kappa = 0,\, \nu^*) = -(x_k^2 + \sigma^2)$$
$$- \delta_k \frac{1-\delta_k^{\vartheta_k'[\mathrm{mod}\ T_v]}}{1-\delta_k}\,(x_k^2 + \sigma^2)$$

while

$$W_k(y;\, \kappa = 1,\, \nu^*) = -x_k^2 - c$$
$$- \delta_k \frac{1-\delta_k^{\vartheta_k'[\mathrm{mod}\ T_v]}}{1-\delta_k}\,(x_k^2 + \sigma^2).$$

But $c > \sigma^2$. Hence $\kappa(y) = 1$ is never optimal.
Now let $y = (1,\, \vartheta)$. Then $\kappa(y) = 1$ and given $\underline{\mu}(\Theta(\vartheta)) \geq 0$ it follows that $\Sigma_i\, v_i^* > (n-1)/2$, which implies $y' = (1,\, \vartheta')$, $\pi_k(\kappa(y) = 1,\, \nu^*(y,\, \eta)) = -c$,

$$W_k(y';\, \kappa = 1,\, \nu^*) = -\frac{1-\delta_k^{\vartheta_k'[\mathrm{mod}\ T_k]}}{1-\delta_k}\, c,$$

while $\kappa(y) = 0$ implies $y' = (0,\, \vartheta)$ with

$$W_k(y';\, \kappa = 0,\, \nu^*) = -\frac{1-\delta_k^{\vartheta_k'[\mathrm{mod}\ T_k]}}{1-\delta_k}\,(x_k^2 + \sigma^2).$$

Further, given $\nu^*(y,\, \eta)$ and $\kappa(y) = 0$, we have $p = -\dot{\omega}$, which implies $\pi_k(\kappa(y) = 0,\, \nu^*(y,\, \eta)) = -x_k^2 - \sigma^2$. But by assumption, $x_k^2 - \sigma^2 \geq c$. Hence, $\kappa(y) = 1$ is optimal. Note that this holds for all $T_k \geq 1$.

Finally we show necessity. So let $\underline{\mu}(\Theta(\vartheta)) < 0$. Then in some period $\vartheta^{\sim}$, $\mu(\vartheta^{\sim}) < 0$ and thus $\Sigma_i\, v_i^* \leq (n - 1)/2$, which implies $y' = (0, \vartheta^{\sim\prime})$.
But then

$$W_k(y'; \kappa^*, \nu^*) = -\frac{1 - \delta_k^{\vartheta_k'[\mathrm{mod}\ T_k]}}{1 - \delta_k}(x_k^2 + \sigma^2),$$

and $\kappa(y^{\sim}) = 1$ cannot be optimal. But then in the period prior to $\vartheta^{\sim}$ $\Sigma_i\, v_i \leq (n - 1)/2$ and the equilibrium unravels by backward induction.

COROLLARY 1. *Let* c $> \sigma^2$. *Given an infinite horizon, the following is true:*

i. For all ϑ, t, t′,

$$\mu(\vartheta^{\mathrm{t}}) = \mu(\vartheta^{\mathrm{t}'}) =: \mu(\vartheta).$$

ii. Majority deference is an equilibrium for an initial distribution ϑ *if and only if* $\mu(\vartheta) \leq \sigma^2$.

Proof

i. $\lim \gamma_i(\vartheta_i) = \lim_{\theta_i \to \infty} (\delta_i - \delta_i^{\vartheta_i})\sigma^2 - (1 - \delta_i^{\vartheta_i})x_k(x_k - 2x_i) = \delta_i\sigma^2 - x_k(x_k - 2x_i)$.

Then $\mu(\vartheta^t) = \text{median}\ \{\gamma \in \Re | \gamma = \delta_i\sigma^2 - x_k(x_k - 2x_i)$ for some $i = 1, \ldots, n\} =: \mu(\vartheta)$.

ii. By upper-hemicontinuity of the Nash correspondence and Theorem 1.

COROLLARY 2. *Let* c $> \sigma^2$. *Given an infinite horizon and a common discount factor* δ, *then majority deference is an equilibrium if and only if* $\delta \geq \frac{\mathrm{x}_\mathrm{k}^2}{3^2}$.

Proof. If $\delta_i = \delta_j$ for all i, j, then

$$\text{median}\ \{\gamma \in \Re | \gamma = \delta_i\sigma^2 - x_k(x_k - 2x_i) \text{ for some } i = 1, \ldots, n\} = \delta\sigma^2 - x_k^2$$

From Corollary 2 and Theorem 1, for majority deference to be an equilibrium one needs $\delta\sigma^2 - x_k^2 \geq 0$.

Notes

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1. This is true even in cases where there are constitutional constraints on the organization of assemblies, as in Italy or Germany, since qualified majorities can typically alter the constitution.
2. See Milgrom, North, and Weingast 1990 or Calvert 1992. For an overview and concise statement of this research program, see Calvert n.d.
3. See, however, Shepsle and Nalebuff 1990, as well as McKelvey and Riezman 1992, on the seniority system.
4. It is true that the floor majority typically defers to the recommendation by the Rules Committee. However, since the floor can always amend a rule proposal, one has to explain why it rarely exercises this option.
5. While the results critically depend on the assumption of risk aversion, I assume quadratic utilities only in order explicitly to characterize the equilibria conditions.
6. Gilligan and Krehbiel assume that the floor can (pre) commit to a closed rule.
7. Signaling games typically have multiple equilibria. Previous research has either focused on the most informative equilibrium or used refinements to eliminate implausible equilibria (Gilligan and Krehbiel 1989).
8. An interesting case in this context is the Italian parliamentary committees' power to *enact* legislation without any consideration of the floor. However, the parent body may easily move to discharge the committee and consider the bill on the floor (D'Onofrio 1979).
9. All theorems should be interpreted with this qualification in mind.
10. For a formal statement and the proof, see the Appendix.
11. Note that the value of T_k is irrelevant for the result.
12. These shocks may influence both retirement and reelection. Examples include scandals, redistricting, adoption of term limits, career improvements for retired representatives, and so forth.
13. Corollaries 1 and 2 in the Appendix present a formal analysis.
14. A formal analysis of the following remarks can be found in Diermeier 1993.
15. A noteworthy aside: at the end of the century, the expected number of terms was still around two, which according to the model is too low to sustain strong committees, while by 1910 the number increased to about three, which is enough to sustain institutional commitments (Polsby 1968).
16. Note that while the staggered terms in the Senate should facilitate the development of norms, the fact that due to possibility of filibuster, the Senate uses de facto a three-fifths majority would make it more difficult to sustain norms.
17. Sinclair (1989) suggests an evolutionary interpretation of an infinitely repeated two-person prisoner's dilemma (see Axelrod 1981), where norms are sustained by the use of Tit-for-Tat strategies. However, even if one ignores the difficulties of generalizing from a two- to an *n*-player prisoner's dilemma, this approach has severe shortcomings in explaining the collapse of norms. Axelrod's theorem 8 states that a population of "nice" strategies, like Tit-for-Tat, that cannot be invaded by a single strategy cannot be invaded by a cluster of "mutant," noncooperative strategies. But then, the apprenticeship norm either should have collapsed if only one new senator did not cooperate or, if it survived, should have remained stable even if clusters of noncooperative members entered the Senate.
18. An example are parties in the German Bundestag, where party members typically defer to the leadership (*Fraktionsvorstand*) (Ismayr 1992).
19. Note the similarity to search committees in the academic recruitment process.

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THE LEGITIMACY OF THE COURT OF JUSTICE IN THE EUROPEAN UNION: MODELS OF INSTITUTIONAL SUPPORT

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Using a survey of mass publics, we investigate the political legitimacy of the Court of Justice of the European Communities. To what degree does the Court have the visibility and diffuse support necessary for legitimacy? What accounts for variability in support for the Court? Are theories developed largely in the American context generalizable in Western Europe to a transnational legal institution? Do the sources of the Court's legitimacy vary across nations, and how? Our analysis indicates that relatively obscure institutions such as the Court of Justice are unlikely to build support through satisfying their constituencies' demands. Without information about the Court of Justice, ordinary citizens form their views based on its connection with the European Union and its association with broad political and legal values. As the Court moves into the limelight of European law and politics, the decisions the judges make may increasingly shape citizens' perceptions of its legitimacy.

The emergence of transnational judicial institutions provides one of the more compelling manifestations of the irrelevance in today's world of bright-line distinctions among domestic, national, and international political, economic, and legal processes. One of these institutions, the Court of Justice of the European Communities, already wields significant transnational political and legal power (Shapiro 1992; Shapiro and Stone 1994).[1] From arcane commercial and financial practices to commonplace but tremendously salient issues, such as whether people can shop on Sunday and gender equality in the workplace, the Court of Justice has emerged as one of the world's most powerful transnational courts (Arnull 1991; Burley and Mattli 1993; Volcansek 1992).

Yet the Court of Justice, like other institutions of the Union, may well suffer under a *democratic deficit*, a shortfall of institutional legitimacy within the mass publics of Europe.[2] The lack of a direct electoral connection, the inability to share in the legitimacy of the nation-state, frequent adjudication of conflicts between national and European Union (EU) law, and the traditionally heavy reliance of legal institutions on the goodwill of ordinary citizens all create important problems of institutional support, legitimacy, and ultimately compliance (Rasmussen 1986; Weiler 1991, 1992, 1994). This deficit of democracy could well translate into a bankruptcy of legitimacy as the Court seeks to maintains its position at the center of law and politics in the EU.

Accordingly, we have conducted an empirical investigation of the levels and sources of the Court's legitimacy among the mass publics of the EU. How visible is the Court of Justice? What levels of specific and diffuse support do mass publics extend to the Court of Justice? What accounts for variations across individuals in diffuse support for this judicial institution? In addressing these empirical questions, we test several conventional theories of institutional support within the transnational context of the European Union.

Quite apart from the obvious political centrality of the Court of Justice in the politics of the European Union, our undertaking is important for several reasons. The European Court of Justice provides a rare opportunity to examine the development of nascent support for a relatively new legal institution. No one has yet collected or examined systematic evidence on the mass legitimacy of this institution. Second, seldom do scholars study the legitimacy of legal institutions in nations other than the United States (see Tate 1987; Tate and Vallinder 1994), even though the mature and emerging democracies of Europe exhibit a wider range of experience in the behavior of courts and judges, from consistent passivity in some to a long record of aggressive judicial action in others. Finally and equally important, we present the results of one of the first empirical studies of the growth of institutional legitimacy outside the confines of the nation-state.

Our results suggest several broad conclusions. For relatively obscure institutions such as the Court of Justice, a strategy of building support through satisfying the demands of various constituencies is unlikely to be successful. Too few of the citizenry have even a vague idea of the general course of the Court's decisions. In the absence of information about the Court of Justice, ordinary citizens depend on this institution's connection with the EU, as well as its association with broad political and legal values such as the rule of law and individual liberty. In other words, lacking specifics, people make inferences and draw conclusions about particular institutions based on their general values and preferences. The intensification of European integration in the late 1990s will, we think, move the Court increasingly into the limelight of European politics. In a richer informational environment, the decisions the judges make may well shape the bases of public commitment to this important institution.

LEGITIMACY AND DIFFUSE SUPPORT: THEORETICAL PERSPECTIVE

Political legitimacy involves a relationship between citizens and government and normally implies the authority to govern, to make authoritative decisions in a polity. Citizens cede authority to the state and its institutions based on a normative judgment about the appropriateness of this relationship between the government and the governed. Scholars often equate political legitimacy and political support, and we have found it useful to do so as well. It is also common to distinguish between two types of support: diffuse and specific. *Specific support* refers to a set of attitudes toward an institution based upon the fulfillment of expectations of policies or actions.[3] *Diffuse support* is a more difficult concept (Easton 1965, 1975), since every citizen will at times disagree with the policies, dislike or distrust the incumbents, or criticize the procedures of political institutions. Normally, however, discontent with officials, policies, or procedures does not translate into alienation from an institution. Citizens may disagree with what an institution does but nevertheless continue to concede its authority as a political decision maker. This firmer and more durable set of attitudes toward the legitimacy of the institution is what we mean by diffuse support.

This conceptual approach proved useful in earlier work (Caldeira and Gibson 1992; Gibson and Caldeira 1992; see also Tyler and Mitchell 1994). We conceptualized diffuse support for the Supreme Court of the United States as the willingness to accept, make, or countenance major changes in fundamental attributes of how the high bench functions in the American constitutional system (see Patterson, Hedlund, and Boynton 1975). We operationalized commitment to the institution along several dimensions: willingness to take away the Court's essential political powers, to limit its supply of controversial cases, and to defend it against political attack. This conceptualization and measurement permitted us to capture the enduring component of public evaluations of the Supreme Court.

Our conceptualization of diffuse support implies a concern for the linkage between legitimacy and compliance. Institutions with adequate legitimacy find that their decisions, even their unpopular decisions are accepted by their constituency and consequently that compliance is more likely (e.g., Gibson and Caldeira 1995). Institutions with this sort of reservoir of favorable opinion are thus more effective not just at making public policy but at implementing it as well.

Diffuse support is, by definition, a relatively stable attitude, but we do not conceptualize it as obdurate. And though our analysis is only cross-sectional, in fact, one of our broad purposes here is to give an account of how the Court of Justice has acquired—and will acquire—legitimacy over an extended period of time. This requires a general model of how people come to have attitudes toward institutions like the European Court of Justice. "On-line" models of political evaluation provide the best account of the development of attitudes toward institutions low in salience among the citizenry (e.g., Taber, Glathar, and Lodge 1994), of which the Court of Justice is a prime example. The formation of attitudes requires information. Most of the time, people have neither the motivation nor the resources to acquire new information about institutions such as the Court of Justice. Political events (e.g., controversy over the Maastricht Treaty on European Union, TEU) or actions by the judges themselves may trigger interest or perhaps lower the level of resources required to gather information about the Court of Justice. According to Taber, Glathar, and Lodge, citizens form evaluations of the institution "immediately upon exposure to information" about events or judicial decisions and then "immediately integrate the affective charge into a running tally" about the institution (ibid., 9). But the information is not, of course, inscribed on a *tabula rasa*. Existing commitments to basic political values loom especially large when citizens are called upon to evaluate relatively obscure political institutions. Soon after updating the evaluation of the Court, an individual might well forget the information on which he or she based the new impression. What remains is a residue, a generalized affective orientation toward the institution.

Diffuse support does not, of course, develop in a vacuum, so we need to provide a sense of the Court's context. We shall briefly sketch (1) the institutional framework within which the Court of Justice operates, (2) examples of critical doctrines and decisions, and (3) contemporary debates about the legitimacy of this institution.

THE COURT OF JUSTICE OF THE EUROPEAN COMMUNITIES

The Court of Justice acts variously as a court of international law, a court of review, a court of appeal, and a court of referral, often in the same disputes (Brown 1989; Lasok and Bridge 1987; Krislov, Ehlermann, and Weiler 1986). Under Articles 169 and 170, for instance, the Court rules on whether members have failed to fulfill obligations set out in the various treaties. Under Article 173 and its decisions, member states, the Council, or the Commission may ask the Court to consider the legality of acts of the Commission, Council, or Parliament. Under Article 177, a national court may request a preliminary ruling from the Court of Justice on the meaning of the Maastricht Treaty or the acts of the various institutions of the EU, enabling national courts to render judgments under an authoritative interpretation of Union laws.

These and related procedures can have dramatic political consequences. Some permit the Court of Justice to strike down the laws of member states or the Union; others give access to the Court to individuals and organizations. The Court itself has aggressively molded the various treaties and laws of the Union into a constitutional charter (Bermann 1989; Cappelletti and Golay 1986; Hartley 1986; Koopmans 1986; Lenaerts 1990; Mancini 1989). Three doctrines stand out: *direct effect* (legal norms of the Union are the law of the land, if "clear, precise, and self-sufficient"), *supremacy* (the Treaty and Union laws and norms supercede domestic law), and *implied powers* (the use of nondelegated powers is permissable if required to achieve the Treaty's purposes) (Weiler 1991, 2413–17). In the late 1960s, the Court even began to check the laws of the Union for violations of fundamental *human rights*, despite the lack of a bill of rights in the Treaty (Frowein, Schulhofer, and Shapiro 1986; Weiler 1986). The Court of Justice has not only established the supremacy of Union law but also simultaneously augmented the power of the national courts by giving them the authority to invalidate national legislation on the basis of Union law. By the standards of national high courts, the Court of Justice has often played a central role as a policymaker; by the standards of transnational courts, it has adopted an aggressive posture during much of the Union's history, checking the Union's own institutions and bringing the parliaments, courts, and bureaucracies of the member states into line with European law.[4]

The Legitimacy of the Court of Justice

Transnational political institutions cannot rely upon the "presumption of legitimacy" associated with national institutions. Since nation-states enjoy a higher level of allegiance from citizens than do transnational polities, the most direct and common source of legitimacy for institutions such as the Court of Justice is either not available or significantly diminished. Moreover, since these institutions are relatively new, tradition cannot sustain them. In addition, the structure of decision making in the EU, dominated as it has been by the consensual style of the Council of Ministers, permits checking by minorities; and the Court of Justice in its decisions often aids antimajoritarian views—that is, those who lose in other political arenas. The result is often laudable from a "European" standpoint, but it is displeasing to the majority and naturally raises questions about the legitimacy of this transnational tribunal.

The basic law of the Union does provide for judicial review, but scholars and judges have worried especially about the implications of judicial activism for the legitimacy of the Court of Justice (e.g., Rasmussen 1986). Much of the worry stems from the aggressive activism of the Court of Justice, beginning in the late 1950s and accelerating in the next 30 years (see Rasmussen 1986, 1988). This style of activism, as exemplified in the doctrines mentioned earlier, has also served to exacerbate the democratic deficit.

To make matters worse, the Court of Justice's authority to strike down the laws of its member states as well as the laws and actions of the Union does not rest on a long tradition of politically powerful judges. Since the end of World War II, many European nations have adopted judicial review or begun to practice it more aggressively; but the national courts of the EU still vary widely in their commitments to judicial review (see Cappelletti 1989). The weak allegiance to judicial review in several of the member states makes it unlikely that the Court can "borrow" legitimacy from national judicial institutions. Indeed, judicial activism at the level of the Court of Justice poses a threat to the supremacy of national laws and judicial interpretations of them.

The existence of the "Constitution" of the EU and the option of referring cases to the Court of Justice may empower judges in national courts to make far-reaching decisions (e.g., Schermer 1990). Thus, judges in national courts may use the laws of the Union to create rights and obligations not available under domestic law or constitutional texts. This empowering of national judges often puts both the national courts and the Court of Justice in the midst of political controversy, which does little to enhance institutional support.

Thus, the legitimacy of the Court of Justice is not only an important theoretical issue for political scientists, as we suggested, but also a matter of contemporary relevance to political debates within the EU.

THE ETIOLOGY OF DIFFUSE SUPPORT

How do institutions acquire diffuse support? Prior research on courts in the United States, together with our theoretical orientation on the sources of attitudes, provides a number of leads to pursue in our analysis. We now explicate a series of explanations tailored to account for the sources of support for the Court of Justice.

Specific Support for the Court of Justice

One solution to this puzzle is simple: citizens come to accept political institutions if they are perceived to function well. In the long term, specific support—satisfaction with policy outputs—evolves into diffuse support (e.g., Loewenberg 1971). We should therefore look for sources of commitment to the Court of Justice in public evaluations of the institution's general performance and its specific outputs. Citizens develop a general evaluation of the Court of Justice based on impressions formed from particular decisions or sets of decisions. This line of argument dominates the literature on public opinion and the Supreme Court. Scholars argue for a strong connection between approval or disapproval of decisions and diffuse support for the Court; those who, on balance, favor the Court's decisions will, over time, accord it high levels of diffuse support (e.g., Adamany and Grossman 1983; but see, for some contrary evidence, Caldeira and Gibson 1992; see also Murphy and Tanenhaus 1968). If this perennial argument applied to the European Court of Justice, those who have been pleased with its outputs would lend it strong support.

Decisions of the Court of Justice do not affect all nations, social classes, occupations, economic sectors, or demographic groups equally (e.g., Lange 1992; Shapiro 1992; Smith and Wanke 1993). Those who directly suffer or benefit from its decisions may have strong reason to form an orientation one way or the other toward the Court of Justice. The Court may generate support by producing favorable public policies over a period of time for particular constituencies. Thus, we hypothesize significant variations in diffuse support for the Court of Justice across social classes, unionized versus non-unionized labor, and gender. For example, in the field of gender discrimination, the Court of Justice has made many important decisions in favor of the interests of women. We would, accordingly, expect women to be more supportive of the Court of Justice if the hypothesis of self-interest were correct.

This proposition obviously cries out for some specific examples and hypotheses. It is not, however, obvious that the Court is perceived as consistently favoring particular political, social, or economic interests in the EU. Thus, Lange has written, ". . . it seems both naive and ahistorical to ignore the substantial dissonance of interest within classes, and the possible confluence of interest across classes . . . within and across nations in an environment of intense economic competitiveness and rapid economic change" (1992, 238). Propositions based on self-interest strike us as plausible, but there is ample reason to doubt whether conventional cleavages within the mass public apply in the case of attitudes toward the Court of Justice.

Attitudes toward the European Union

The Court of Justice is, first and foremost, a European institution. We would, accordingly, expect attitudes toward the Union to go some way toward predicting diffuse support for the Court of Justice.

Politics and political institutions for many citizens, perhaps a majority, do not figure as salient objects of thought and discussion. Bereft of information about specifics, members of the mass public who do not pay much attention to politics might well form orientations toward particular institutions from general impressions of the political system. Citizens' views would likely move from the general to the particular rather than the other way around. That is, citizens deduce their views of particular institutions from their more general affect toward the polity. Indeed, in the American setting, we have some evidence in favor of precisely this process: some citizens do not distinguish among the three branches of the national government and they evaluate the Supreme Court on the basis of more general feelings toward the party in power (e.g., Murphy and Tanenhaus 1968; Murphy, Tanenhaus, and Kastner 1973, 27). In the context of Europe, we might also expect some to judge the Court of Justice on the basis of more firmly-held opinions about the EU (Gabel 1994; Janssen 1991; Kuechler 1991; Nierdermayer 1991), especially since the Court is perhaps even more a creature of the EU (given how the member states select its judges and the lack of constitutional protections of its prerogatives) than the Supreme Court is of the U.S. government. We could, in consequence, reasonably anticipate a significant relationship between favorable attitudes toward the Union and commitment to the Court of Justice.

Earlier we commented on the concerns in Europe about a "democratic deficit" in the Union's institutions. If, as we argue, citizens link the Court and the Union, those who see a deficit in democracy in general within the EU might exhibit a weaker commitment to what is, arguably, the EU's least accountable institution, the Court of Justice.

Political and Legal Values

Quite apart from evaluations of the recent outputs or general performance of an institution, people may favor or disfavor particular institutions based on the basic political values about which these citizens have formed judgments. Citizens who have little specific information about the past and present activities of an institution may nonetheless judge it based on a general sense of whether the institution advances those values. This is especially so for courts, since law and courts play such a highly symbolic role in most polities. For instance, Caldeira and Gibson (1992) report that citizens who placed a high value on the rule of law and liberty (values generally compatible and associated with courts as institutions) were more likely to support the Supreme Court of the United States (for a review, see Caldeira 1990; Murphy and Tanenhaus 1968, 1990). In those analyses, support for the Supreme Court did *not* reflect satisfaction with policy outputs but, instead, seemed to derive from basic value orientations such as support for individual liberty and commitment to democracy. Similar processes might well characterize the relationship between the Court of Justice and the mass publics of the Union. Here, we focus on support for the rule of law, the relative valuation attached to individual liberty when it conflicts with social order, alienation from law, and ideological self-identification. Each of these requires explication.

Willingness to tolerate exceptions to the law is a fundamental value. Nearly everyone agrees that there are some circumstances under which law must be put aside in favor of justice or self-interest, just as nearly everyone agrees that under most circumstances the universal and equal application of the law should prevail. Between these two extremes, people differ in the rigidity with which they insist on adherence to law. We hypothesize that those who are more universalistic in their attitudes toward the rule of law will be more likely to support the Court of Justice, whose role it is, after all, to apply general law to specific cases and to ensure some degree of uniformity in law throughout the Union.

The willingness to tolerate disorder for the benefit of individual liberty, or, conversely, the willingness to sacrifice liberty for the sake of social order, constitutes another basic distinction among people (Caldeira and Gibson 1992). It is a fundamental social attitude, stable over time and closely associated with a variety of other political beliefs. Since, in constitutional democracies, courts pay close attention to issues of individual liberty—even if their decisions do not exclusively favor liberty interests—we hypothesize that those who value individual liberty more highly than social order will be more likely to extend support to the Court of Justice. Courts are, after all, the arena in which individuals can gain access to the policymaking process, resolve a dispute, or seek redress of grievance without the intermediation of organized interest groups or other institutions.

For some citizens, law is a liberating force, either because it creates or reinforces a desirable social order or because it serves other interests of the citizen. Others, however, may perceive law as a repressive, coercive force. Instead of representing some sort of social consensus, of which the citizen is a part, law may be seen as an instrument of social control. We term this "alienation from law." We hypothesize that those who see law as a liberating force will lend greater support to the Court of Justice.

More traditional political views may also affect willingness to support the Court of Justice. In particular, a citizen's perceptions of where he or she falls on the left-right spectrum, which is an important determinant of many political behaviors and opinions in Europe (e.g., Lewis-Beck 1988; Ingelhart 1991), might shape views of the Court of Justice. To the extent the Court of Justice makes policies associated with one end of the ideological spectrum (although, as we said earlier, we do not necessarily assert that it does), citizens may apply standards of judgment similar to those used in evaluating other political objects.

Cognitive Mobilization

The extent to which attitudes toward the EU account for support for the Union's high bench surely depends in part on the level of awareness of the Court of Justice. As we shall see, not all of the citizens are aware of this institution; and among those who are, the Court's salience varies a great deal. The Court of Justice, like most judicial institutions, is relatively obscure and not easily accessible to ordinary citizens.

More generally, of course, citizens vary in level of awareness about and attention to politics and government. Cognitive mobilization—the degree to which an individual citizen is "integrated into modern organizations and extensive communications networks" (Inglehart 1977; Inglehart 1990, 377)—contributes to social learning, learning typically supportive of the dominant values, expectations, and institutions of a political community (McClosky and Zaller 1984, 12). Those who possess a greater store of political information (i.e., the "cognitively mobilized") should, in general, have greater awareness of the Court and more favorable attitudes toward it. We therefore hypothesize that cognitive mobilization contributes to support for the Court of Justice.[5]

Minimally, cognitive mobilization involves paying attention to the mass media; at a more significant level, it involves active consideration of politics through attempts to discuss politics with others. Opinion leaders are likely to have undergone more social learning, so that their views are typically more consonant with the dominant norms of the polity (e.g., McClosky and Zaller 1984, 9). We therefore hypothesize a connection between support for the Court and consumption of media and opinion leadership, two of our indicators of cognitive mobilization.

Cognitive mobilization requires skills and motivation (to move a step back in the causal chain). Educational attainment is one good indicator of a citizen's skills. Thus we hypothesize a positive relationship between support and educational attainment and knowledge of the Court of Justice.

Thus far, we have characterized the relationship between cognitive mobilization and diffuse support for the Court of Justice as a direct effect: mobilization leads to higher levels of exposure to positive information and therefore to higher levels of support. Yet we think it is equally plausible to imagine cognitive mobilization conditioning the impact of other variables in our model of diffuse support. For example, for those who rank high in awareness of the Court of Justice, specific support might make a larger difference than for those who score lower. The mobilized would pick up more information about specific decisions and policies and therefore be better able to link specific support to institutional evaluations. Moreover, we need to watch for nonlinearities. Perhaps, as we sometimes observe in models of political persuasion, specific support and other variables exert a weaker pull on those who fall at the extremes and stronger on those in the middle on levels of awareness of the Court of Justice.

Summary

We have offered four sets of explanations for diffuse support: specific evaluations of the Court and its policies, support for the EU, basic political and legal values, and cognitive mobilization. These are the hypotheses we will submit to empirical testing.

DESIGN OF THE RESEARCH

We base our analysis on surveys conducted in each of the member states of the EU between 21 September and 15 October 1992. We commissioned several questions concerning the Court of Justice as part of

Eurobarometer 38.0. The Eurobarometer represents the population of the respective nationalities, aged 15 years and over, in each of the 12 member states. The basic sample design, applicable in each of the countries, is a multistage probability sample, with random selection of respondents.[6]

The Eurobarometer routinely provides separate samples of approximately a thousand respondents in Germany. Since there are such large differences between East and West Germans, we treat these samples as if they were from different nations. The Eurobarometer also provides a sample of about three hundred residents of Northern Ireland as a supplement to the sample of approximately a thousand from Great Britain. The two samples are quite different, so we deem it inappropriate to treat Northern Ireland as simply an extension of Great Britain. Thus we report the results for Northern Ireland separately, even while recognizing that the size of the sample makes inferences to the total population inefficient.

The Political Context of the Survey

Our survey went into the field in late September of 1992, a period of considerable turmoil in the politics of the Community. During the survey, the European Community had become especially salient, based in part on the votes to ratify the Maastricht Treaty in Denmark, Ireland, and France. The Treaty, negotiated in 1991, was to have been the precursor to the United States of Europe. Instead, the Treaty acted as a magnet for a panoply of anti-Europeanists, drawing from both the left and the right. The debate over the Maastricht Treaty and the referenda no doubt raised the salience of all Community institutions; and a large portion of the mass public seemed to resent the continuous transfer of discretionary policymaking power to the Commission in Brussels (Goldstein 1992, 123). Perhaps the most important threat to the generalizability of our survey results is thus the fallout from the Maastricht controversy. Here we simply note this context; later, we shall consider the implications of the timing of our survey.

THE SALIENCE OF THE COURT OF JUSTICE

For any political institution, visibility among the mass public constitutes a critical first step toward achieving political legitimacy. How can a citizen who does not know of an institution, at least in some minimal sense, provide diffuse support? For judicial institutions, most of which lack a direct popular linkage, it is difficult to gain the requisite public attention (e.g., Kosaki and Franklin 1991). The Court has recognized the problem of anonymity and now engages in a wide range of public relations activities. Accordingly, we begin our analysis by probing the evidence on the Court of Justice's institutional salience in some detail.

How visible is the Court of Justice among the mass publics of Western Europe? Table 1 reports the responses to several questions about EU and national institutions. We asked the respondents if they had "seen or heard, in the papers, on the radio, or on the television" anything about the institution. Not at all surprisingly, the Treaty of Maastricht generated the highest level of recognition among the respondents. In France nearly everyone had heard of the Treaty (the referendum on the Treaty had just taken place at the time of our survey), and even in Italy, where awareness is least common, three out of every four respondents were aware of the Treaty. Not at all surprisingly, the Community itself is also widely recognized among Western Europeans.

It is clear that the European Court of Justice is not the most salient EU institution. In most countries, the Commission and the Parliament are of roughly equal salience, with the largest gap between the two institutions found in Italy (where the Parliament is more salient). Still, the Court is remarkably well known. In Denmark, Ireland, and Northern Ireland, the Court is more widely recognized than the European Parliament; and the difference in salience between the Court and Parliament is trivial in Great Britain, as well. Perhaps surprisingly, more than one-half of the respondents had heard something of the Court in Denmark, Great Britain, and Ireland. The considerable salience of the Court in Ireland and the United Kingdom probably reflects the widely reported dispute over a young Irish girl's legal difficulties connected to her efforts to travel to Britain for an abortion, a matter many people expected the Court of Justice to enter (Girvin 1994). Compared to the national high court, the Court of Justice is typically less well known (except in Luxembourg, home to the Court), but the differences are not nearly as great as we had anticipated.

Yet lack of recent awareness should not necessarily be taken to imply total ignorance of the Court. For instance, among Danes who reported that they had heard nothing of the Court recently, over 20% professed to be either very or somewhat aware of the Court.[7] Table 2 reports the levels of awareness of the Court for all respondents in all countries. Clearly, not many Europeans know a great deal about the Court of Justice. Across the Community as a whole, only 4.5% claim to be very aware of the Court. In some countries (Portugal, Spain, and Italy) nearly a majority of the respondents assert that they had never heard of the Court before the interview. Yet there is also evidence that the Court is at least moderately well known in several countries. Nearly everyone in Denmark knows something about the Court, and in Ireland and Great Britain awareness is fairly substantial. Even in Greece, at the periphery of the EU, about one-fourth of the respondents claim to be either very or somewhat aware of the Court. These data suggest that Court has become something of a public institution, one that no longer works entirely in anonymity and obscurity.

Generally, then, the Court of Justice has a moderate degree of visibility within the mass publics of the

TABLE 1

Salience of European Community Institutions, 1992

MEMBER STATE	PERCENTAGE HEARING SOMETHING RECENTLY ABOUT					
	EUROPEAN COMMUNITY	EUROPEAN COMMISSION	EUROPEAN PARLIAMENT	COURT OF JUSTICE	TREATY OF MAASTRICHT	NATIONAL COURT
Belgium	79.3	55.2	52.9	31.2	82.9	42.4
Denmark	89.1	67.8	59.0	63.4	94.5	74.7
France	83.3	55.6	57.7	26.7	96.2	36.8
Germany (West)	84.3	52.0	57.0	40.4	82.7	69.1
Germany (East)	86.1	51.6	52.8	31.4	83.0	77.9
Great Britain	79.9	49.8	53.4	52.1	89.0	63.2
Greece	82.4	49.9	57.2	45.8	87.3	82.7
Ireland	73.5	54.5	50.6	51.1	87.0	72.8
Italy	80.1	42.8	53.7	21.0	74.2	63.2
Luxembourg	84.5	61.9	65.0	43.9	93.5	33.7
The Netherlands	86.7	51.6	50.5	22.7	90.8	37.3
Northern Ireland	64.3	41.7	41.4	46.2	80.4	57.7
Portugal	83.7	61.1	61.6	34.5	78.1	52.9
Spain	75.4	53.2	55.9	26.9	81.2	57.6
European Community	81.6	51.3	55.3	34.4	84.9	59.0

Note: For the wording of the questions, see the Appendix.

EU. Surely it is not as well known as the European Parliament, and awareness generally lags behind national courts, but the Court is far from invisible. Furthermore, in light of the growing salience of all EU institutions, we expect the visibility of the Court to increase over time.

Substantial portions of the samples claim no awareness of the Court of Justice. For these people, it makes little sense to accept their responses to the rest of the questions about the Court since they are responding to an institution about which they had only just learned. Their answers may not be entirely void of substantive content, but it is prudent to separate them from the remainder of the respondents in the analysis that follows (see Murphy, Tanenhaus, and Kastner 1973, for use of a "culled sample").

We shall report a measure of diffuse support for the Court of Justice based on a three-item index. We collected responses to each of these items on a five-point response set, with the middle category

TABLE 2

Awareness of the European Court of Justice, 1992

MEMBER STATE	LEVEL OF AWARENESS					
	VERY	SOMEWHAT/ NOT VERY[a]	NOT AT ALL	THREE DON'T KNOWS	% EXCLUDED	TOTAL N
Belgium	2.1	71.3	26.6	23.0	38.7	1,040
Denmark	3.4	94.4	2.2	4.6	6.8	1,000
France	.5	62.6	36.9	13.9	42.4	1,005
Germany (West)	6.2	85.3	8.5	12.0	17.9	1,013
Germany (East)	1.9	78.6	19.5	9.5	23.9	1,015
Great Britain	13.8	68.2	18.0	12.7	24.4	1,058
Greece	3.4	67.7	28.9	21.0	38.7	1,006
Ireland	17.8	65.5	16.7	16.9	24.7	1,008
Italy	.9	54.6	44.6	21.3	51.3	1,052
Luxembourg	3.3	74.2	22.5	17.7	29.1	500
The Netherlands	1.9	75.4	22.7	9.0	25.5	1,003
Northern Ireland	8.5	66.4	25.1	26.7	39.8	304
Portugal	2.4	49.8	47.8	28.4	54.0	1,000
Spain	.4	54.2	45.4	11.3	49.6	1,004
European Community	4.4	67.5	28.2	14.7	34.8	[b]

Note: For the wording of the questions, see the Appendix.
[a] Includes "don't know" responses.
[b] Weighted to the total population of the European Community.

representing "uncertain" or "don't know." To restrict our analysis to respondents who had some prior knowledge of the Court, we also treat as uninformed those who gave three agnostic responses to our items. Most of these people are those who told us earlier they had no awareness of the Court, but some small fraction of those claiming awareness were nonetheless unable to express a substantive opinion toward the Court. The three percentages (those who were unaware of the Court, those who gave three agnostic responses, and the combined total) also appear in Table 2.

The size of the percentage of respondents excluded from analysis—the *inattentive* public—varies considerably across country, from a high of 54.0% in Portugal to a low of 6.8% in Denmark. Some of the cross-national differences surely reflect the salience of the battle over Maastricht, although the French data (where the Maastricht referendum was not held until September 1992) reveal a large proportion of the mass public essentially oblivious to the Court of Justice. Overall, slightly more than a third of the mass public in the EU has virtually no information or opinion about the Court, and we have consequently separated them from the following analyses. We refer to this culled sample as the *attentive public* within each of the countries (see Murphy, Tanenhaus, and Kastner 1973); and in our analysis of diffuse support, we examine only this group. To be sure, we think the wide range in the size of the attentive public across the Union is one of our more intriguing findings,[8] but this is not the place to pursue a comprehensive explanation of why the percentage excluded varies so considerably across nations.

MEASURING DIFFUSE SUPPORT FOR THE COURT OF JUSTICE

We define *diffuse support* as institutional commitment—a willingness to defend the institution against structural and functional alterations that would fundamentally alter the role of the institution in society (Caldeira and Gibson 1992; Gibson 1989, 1991). This means, at the extreme, a willingness to defend the institution against attempts to abolish it. Thus, one of our propositions read, "If the European Court of Justice started making a lot of decisions that most people disagree with, it might be better to do away with the Court altogether." Those who agreed are obviously expressing low levels of institutional commitment to the Court of Justice. This statement represents the antithesis of diffuse support: it says that the institution should only be supported to the extent it produces satisfactory outputs.

Our second measure of support concerns the basic function of the institution—deciding cases. Those who support this function for the Court only on the condition that the decisions be favorable exhibit little institutional commitment. This idea we measured with the following question: "The right of the European Court of Justice to decide certain types of controversial issues should be reduced." To the extent that respondents are willing to place restrictions on the decision-making authority of the Court, they are expressing low levels of institutional commitment.

Some degree of political independence is an essential aspect of courts. To the extent that courts are subservient to other political institutions, the rule of law is threatened and the distinctive role of courts is compromised. Thus one measure of institutional commitment is the willingness to defend the political independence of the Court. We asked the respondents to react to the following statement: "The political independence of the European Court of Justice is essential. Therefore, no other European institution should be able to override Court opinions even if it thinks they are harmful to the European Community." Those who disagree with this assertion express low levels of diffuse support for the institution. The responses to these items appear in Table 3. We report the replies of only the attentive public.

These data indicate limited legitimacy for the Court of Justice. Overall, about 7% of the respondents endorsed all three statements, 16% gave supportive responses to two of the three items, about 33% expressed support on only one item, and *45% of the respondents expressed support on not a single item*. There is important national variability, with support particularly high in The Netherlands, followed by Italy, and particularly low in Spain, followed by Ireland, Portugal, and East Germany. The "Institutional commitment" rows are especially instructive since the statement presented to the respondent so unambiguously represents our notion of such commitment. Only slightly more than one-quarter of the respondents expressed support for the institution; nearly one-half gave explicitly nonsupportive responses. On this item, the Court of Justice has especially weak legitimacy in East Germany and Spain; and in eight countries a majority of the respondents assert that the Court ought to be abolished if it makes a lot of decisions with which most people disagree. Only in The Netherlands are there more supporters of the Court than opponents. Overall, *the Court of Justice seems to have more enemies than friends within the mass publics of the EU.*

Common factor analysis of the three measures of diffuse support within the attentive public subsample, pooling the data across all countries, produces a unidimensional solution; but the three items are of unequal utility as measures of the latent concept.[9] The third item, judicial independence, particularly worries us. This statement was probably too complicated for most people, so that the responses are contaminated by random error.[10] Since the item is weighted slightly in the construction of the factor scores, there is little contaminating effect on the index. We must be mindful, however, that the dependent variable is essentially comprised of two items, not three. There is considerable national variability in the average response to the Court, ranging from very low support in Spain to substantial support

TABLE 3

Diffuse Support for the European Court of Justice, Attentive Public, European Union, 1992

				INDEX OF SUPPORT		
MEMBER STATE	SUPPORT[a]	UNDECIDED, DON'T KNOW	NOT SUPPORT	MEAN	S.D.	N
Germany (East)						
Institutional commitment	16.8	15.1	68.1	−.164	.693	766
Institutional powers	34.2	29.3	36.5			
Judicial independence	54.2	24.5	21.3			
Ireland						
Institutional commitment	21.8	12.3	65.9	−.151	.671	759
Institutional powers	25.1	32.0	42.9			
Judicial independent	48.1	31.6	20.3			
Northern Ireland						
Institutional commitment	22.2	9.3	68.5	−.113	.652	183
Institutional powers	27.3	26.8	45.9			
Judicial independence	52.2	30.6	17.2			
Spain						
Institutional commitment	22.4	8.1	69.5	−.248	.736	506
Institutional powers	19.3	24.9	55.8			
Judicial independence	44.6	25.5	30.0			
Germany (West)						
Institutional commitment	24.5	20.0	55.5	.042	.721	830
Institutional powers	36.2	33.0	30.7			
Judicial independence	50.0	29.7	20.3			
Luxembourg						
Institutional commitment	26.8	14.5	58.7	.005	.748	355
Institutional powers	32.9	30.8	36.4			
Judicial independence	57.6	28.0	14.4			
Great Britain						
Institutional commitment	29.7	11.3	59.0	−.034	.717	799
Institutional powers	29.1	29.1	41.8			
Judicial independence	48.3	21.1	30.6			
France						
Institutional commitment	30.6	15.6	53.8	−.083	.706	579
Institutional powers	18.7	28.7	52.5			
Judicial independence	50.1	26.0	24.0			
Belgium						
Institutional commitment	31.0	19.6	49.5	.045	.685	632
Institutional powers	27.9	31.9	40.2			
Judicial independence	53.2	29.5	17.3			
Portugal						
Institutional commitment	32.5	8.9	58.6	−.039	.769	460
Institutional powers	33.6	25.1	41.3			
Judicial independence	43.4	19.0	37.5			
Denmark						
Institutional commitment	33.2	12.9	53.9	−.043	.782	923
Institutional powers	22.8	24.3	52.9			
Judicial powers	64.5	20.3	15.2			
Italy						
Institutional commitment	34.0	19.4	46.6	.191	.674	512
Institutional powers	37.1	38.7	24.2			
Judicial powers	46.1	30.7	23.2			
Greece						
Institutional commitment	34.3	14.3	51.4	.009	.782	617
Institutional powers	29.9	28.4	41.7			
Judicial powers	48.1	31.2	20.8			
The Netherlands						
Institutional commitment	48.8	10.3	40.8	.347	.842	747
Institutional powers	42.4	23.2	34.4			
Judicial independence	64.4	15.3	20.2			
European Community						
Institutional commitment	28.9	15.2	55.9	.000	.733	[b]
Institutional powers	30.4	30.5	39.2			
Judicial independence	49.9	26.0	24.1			

Note: The countries are ranked from least to most supportive based on the percentages of respondents expressing support for the Court of Justice on the institutional commitment item. For the wording of the questions, see the Appendix. Each row totals 100%.

[a] For the items on institutional commitment and institutional powers, disagree strongly or somewhat signify support; for the item on judicial independence, agree or agree somewhat signify support.

[b] Weighted to the total population of the European Community.

in The Netherlands. It is this interpersonal and international variability we seek to explain.

MODELS OF DIFFUSE SUPPORT

To the extent diffuse support depends on specific support, a court will have difficulty navigating through times in which it engenders controversy and opposition. This hypothesis is critical to our analysis; accordingly, we first examine this relationship in some detail.

Diffuse and Specific Support

We measured specific support by asking our respondents, "Has what you have heard or read given you a generally favorable or unfavorable impression of the European Court of Justice?"[11] The modal response to this question is uncertainty. Most Europeans simply do not have sufficient information about the Court to judge whether it is performing adequately. Among those with an opinion, however, more expressed a favorable rather than an unfavorable view of the Court. Indeed, the small percentage of people who hold unfavorable attitudes contrasts quite starkly with the relatively low level of diffuse support, leading us to anticipate only a modest relationship between diffuse and specific support.

Table 4 shows the relationship between specific support and a trichotomized version of our scale of diffuse support.[12] A strong correlation between the diffuse and specific support measures would, of course, raise questions about the validity of our measure of diffuse support. If these items were measuring nothing more than satisfaction with the current outputs of the Court, it would be worrisome, since diffuse support is supposed to be support even in the presence of dissatisfaction with policy. As clearly indicated by the gammas, there is not an excessively strong relationship between diffuse and specific support, even though in general the two measures are moderately correlated. This is reassuring.

Yet, diffuse support is not high even when there is favorable specific support. For instance, only in The Netherlands do we find a majority of the respondents with favorable specific support expressing high diffuse support. In Ireland and Spain, in contrast, barely more than one-quarter of the respondents satisfied with Court outputs expresses high diffuse support. These findings reinforce our earlier conclusion that the reservoir of diffuse support for the Court is remarkably shallow; satisfaction with the Court's outputs has not yet been transformed into institutional commitments.

The acid test of the theory of diffuse support is whether those who are *dissatisfied* with the outputs of the institution nonetheless defend its continued existence. In general, the group who passes this test is not large in the EU. For instance, in the worst case, only 7.9% of the respondents in Northern Ireland and 8.9% of the respondents in Denmark who are dissatisfied with the Court express high diffuse support. In general, somewhere around 20% of those who are dissatisfied with the Court's decisions still express high diffuse support. Only in Italy and The Netherlands does a substantial portion of the dissatisfied respondents nonetheless express a high level of support for the Court.

It is also ominous for the Court that those who are unable to judge the current performance of the institution are generally more negative than positive toward the institution as a whole. For instance, only in Italy and The Netherlands do we find a greater percentage of these agnostic respondents expressing higher diffuse support than low support. In most instances, those unaware of the Court's activity are generally unlikely to express support.

From these data, then, we have a picture of an institution without a large bedrock of legitimacy. And even though most view the Court's performance as satisfactory, this satisfaction has not changed into institutional commitment.

Multivariate Results

Table 5 reports the results of regressing our scale of diffuse support on the set of independent variables for which we have provided theoretical justification.[13] Overall, we encounter reasonable predictability in attitudes toward the Court of Justice—the R-squareds range from a low of .12 in Ireland to highs of approximately .30 in Belgium, West Germany, and Northern Ireland. Support for the Court of Justice, in other words, does not appear to vary randomly.

Does self-interest, in the sense of "group benefits," account for support for the Court of Justice? Probably not, at least as we have conceptualized and measured self-interest. In few of the member nations do our indicators of self-interest make a significant dent in support for the Court. For example, in no instance is the effect of union membership on diffuse support statistically significant (and there is even some alternation in the sign of the coefficients). Self-identification with a social class also fails to achieve statistical significance at .001 in any of the countries. We had hypothesized a positive relationship for females because of the important work the Court of Justice has done in the area of gender equality, yet our evidence on this point clearly goes contrary to the proposition. In general, then, we have unearthed little support indeed for the hypothesis that self-interest affects attitudes toward the Court. For our strongest and most plausible hypothesized relationship, between gender and support, we find no evidence.

Thus the Court of Justice has not become politicized in the sense that support or opposition to the Court is dominated by traditional political cleavages. For instance, although social class may well predict certain aspects of European politics, there is little consistent effect on attitudes toward the Court. These findings certainly reinforce Lange's general conclu-

TABLE 4

The Relationship between Specific Support and Diffuse Support for the Court of Justice, Attentive Public, 1992

MEMBER STATE BY DIFFUSE SUPPORT	SPECIFIC SUPPORT			
	UNFAVORABLE	NEITHER/ DON'T KNOW	FAVORABLE	GAMMA
Belgium				
High	23.7	27.3	40.9	.21
Medium	26.9	41.6	30.6	
Low	49.4	31.1	26.5	
Denmark				
High	8.9	20.7	42.8	.45
Medium	25.6	31.0	28.5	
Low	65.5	48.3	28.7	
France				
High	16.0	21.6	39.8	.28
Medium	23.6	36.1	25.8	
Low	60.4	42.2	34.4	
Germany (East)				
High	18.0	19.2	30.4	.24
Medium	24.8	35.6	36.4	
Low	57.2	45.2	33.2	
Germany (West)				
High	21.8	23.9	47.0	.33
Medium	33.5	37.9	29.3	
Low	44.7	38.2	23.7	
Great Britain				
High	12.4	18.7	39.2	.42
Medium	22.4	42.1	34.1	
Low	65.2	39.2	26.7	
Greece				
High	19.4	35.7	42.1	.22
Medium	25.8	27.3	27.7	
Low	54.8	37.0	30.2	
Ireland				
High	13.5	12.8	26.9	.31
Medium	30.4	34.0	37.0	
Low	56.1	53.2	36.1	
Italy				
High	48.1	36.4	46.0	.13
Medium	20.8	41.0	35.0	
Low	31.1	22.6	19.0	
Luxembourg				
High	20.6	24.3	39.5	.21
Medium	32.8	36.2	28.0	
Low	46.6	39.5	32.5	
The Netherlands				
High	36.5	45.3	57.1	.17
Medium	37.6	29.3	20.7	
Low	25.9	25.4	22.2	
Northern Ireland				
High	7.9	16.2	30.7	.34
Medium	27.8	52.9	35.8	
Low	64.3	30.9	33.5	
Portugal				
High	11.8	26.4	35.7	.24
Medium	27.8	26.5	26.5	
Low	60.4	47.1	33.8	
Spain				
High	16.9	15.9	26.0	.14
Medium	31.7	28.4	25.9	
Low	51.4	55.7	48.1	
European Union				
High	53.7	39.0	27.8	.29
Medium	27.0	36.5	30.7	
Low	19.3	24.5	41.5	

Note: Entries are percentages. Within each country, each column totals 100%, except for rounding error.

sion that EU politics has not solidified to the extent that it tracks traditional cleavages of domestic politics.

There is also little evidence for most of our propositions about cognitive mobilization and support for the Court of Justice. The extent of media consumption (a measure of attentiveness to newspapers, television, and radio) has little to do with levels of diffuse support. We do not, of course, have any way of knowing from these data whether the absence of this relationship is a result of mixed or negative messages in the media or simply a faulty proposition. Similarly, we find no evidence in favor of the hypothesis that opinion leaders and the better educated are more supportive of the Court. Neither level of educational attainment nor opinion leadership reaches statistical significance at .001.

One of our two propositions about the connection between attitudes toward the European Union and support for the Court of Justice fares well. Those who express more support for the EU are also likely to extend support to the Court of Justice. In 10 of the 14 nations, support for the Union is associated with support for the Court to some degree or another. For 9 of the nations, the relationship is quite strong ($p < .001$). We see four exceptions: Portugal, Greece, Spain, and Luxembourg. Greece, Spain, and Portugal share at least one characteristic—recency of membership in the EU. Perhaps the linkage among European institutions is not so clear for citizens in these newcomers to the Community. For Luxembourg, perhaps the proximity of the Court to the residents of this small nation makes the Union an unnecessary linkage to the Court of Justice. In many of the nations, the coefficient for support for the European Union is the largest or among the largest. Clearly, then, the connection with the European Union packs a considerable punch on support for the Court.

Perhaps this by itself is not a surprising finding. Indeed, perhaps the surprising finding is that the relationships between support for the EU and support for the Court of Justice are not stronger. For an institution so isolated from the public, one might well expect its connection to the European Union's institutions to account for the bulk of the variance. Yet it appears that, for many respondents, attitudes toward the Court are not a simple extrapolation of attitudes toward the Union as a whole. This may mean that the Court has become visible enough for some to have a differentiated view of the institution. We shall explore this particular finding in greater detail below.

A number of critics of the European Union have made much of the "democratic deficit" of its institutions. If democratic connections are indeed a major means of legitimizing institutions, then the Court of Justice might well suffer a shortful of legitimacy. Contrary to this proposition, the data reveal little or no effect of perceptions of a democratic deficit on attitudes toward the Court. Only in one nation, Portugal, does the coefficient for a perceived democratic deficit in the EU achieve even minimal statistical significance ($p < .01$). For the others, the pattern is clear: those who yearn for greater democracy from the governing institutions of the EU are no more likely to oppose the Court than those who do not want greater democracy in the EU.

Perhaps as much as support for the Union, broad political and legal values do much to shape support for the Court of Justice. Overall, there is a strong and consistent relationship between political values and support for the European Court of Justice. The amount of variance in support accounted for by our four measures *alone* is quite substantial in several countries (e.g., Belgium .24, Portugal .21), moderate in some (France .15, The Netherlands .13), and small in others (Ireland .03, Italy .07). The results naturally vary from country to country, but we feel safe in saying that support is more likely to be found among those with particular political values. It seems that members of the public deduce attitudes toward the Court from more general attitudes toward law and liberty. With a small number of exceptions, at least one of the three values liberty, rule of law, and legal alienation significantly affects diffuse support.

In most of the countries, those who favor the more universalistic application of the law are more likely to support the Court. This includes Belgium, East Germany, West Germany, Greece, Portugal, and Spain at a significance level of .001, and Italy and The Netherlands at .01. The standardized coefficients for the rule of law in several of the nations are quite substantial (e.g., Greece .24, Portugal .20, Spain .22). In general, then, those who are favorably disposed toward universalistic application of law are favorably oriented toward the Court of Justice. Thus, in the eyes of several of these publics, the rule of law and the Court go hand in hand.

In five of the countries, those who are inclined toward liberty even at the expense of disorder are supporters of the Court; and in all five of the nations (Belgium, West Germany, Britain, The Netherlands, and Portugal), the effect is substantial. Perhaps courts, as generally protective of individual rights and liberties, are more likely to draw support from those particularly predisposed to value individual liberty. It so happens that the Court of Justice has actively advanced individual liberty and rights, so in a sense those favoring liberty may be responding to their satisfaction with the outputs of the Court.

Perceptions of law as an agency of coercion make a significant mark on support for the Court in several of the nations in the EU. Those who see the law as standing against their interests are much less likely to support the Court of Justice in Belgium, East Germany, and Portugal. This measure of political values does not carry as much weight as do the other two, but clearly, in some nations such a perception makes a difference in attitudes toward the Court.

Together these broad political and legal values carry a great deal of weight. Yet it is clear that in some of the cases the political and legal values may be sharing roughly the same variance in the dependent variable, depressing the coefficients and increasing the standard errors in some countries. We are especially concerned about the correlation between the

TABLE 5

Models of Diffuse Support, Attentive Public, 1992

	MEMBER STATE						
VARIABLES	BELGIUM	DENMARK	FRANCE	GERMANY (EAST)	GERMANY (WEST)	G.B.	GREECE
Specific support for Court of Justice	.08	.23**	.07	.01	.12**	.24**	.10
	.09	.25	.08	.02	.13	.24	.12
	(.05)	(.04)	(.05)	(.06)	(.04)	(.04)	(.05)
Awareness of Court of Justice	.06	.10*	.09	.14**	.09	.07	.04
	.08	.14	.17	.20	.10	.07	.05
	(.06)	(.05)	(.08)	(.05)	(.04)	(.04)	(.06)
Support for EU	.19**	.20**	.25**	.26**	.24**	.25**	.12
	.16	.15	.18	.20	.18	.16	.10
	(.04)	(.03)	(.04)	(.03)	(.03)	(.03)	(.04)
Support for liberty	.17**	.08	.04	.03	.15**	.12**	.07
	.12	.05	.03	.02	.10	.07	.04
	(.03)	(.02)	(.03)	(.02)	(.02)	(.02)	(.03)
Support for rule of law	.16**	.03	.12	.14**	.21**	.03	.24**
	.10	.02	.07	.09	.13	.02	.16
	(.03)	(.03)	(.03)	(.03)	(.02)	(.03)	(.03)
Alienation from law	.17**	.04	.12	.18**	.09	.04	.02
	.11	.03	.07	.11	.05	.02	.02
	(.03)	(.03)	(.03)	(.02)	(.02)	(.03)	(.03)
Democratic deficit	.02	.02	.02	−.06	−.04	.05	.07
	.01	.03	.01	−.06	−.04	.07	.06
	(.04)	(.03)	(.04)	(.04)	(.03)	(.04)	(.04)
Ideological self-identification	.05	−.03	−.04	−.09	−.05	−.03	.08
	.02	−.01	−.01	−.03	−.02	−.01	.03
	(.01)	(.01)	(.02)	(.01)	(.01)	(.01)	(.02)
Level of education	.12*	.00	.10	.00	.06	.05	−.00
	.03	.00	.02	.00	.01	.01	−.00
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Opinion leadership	−.04	.07	−.02	.08	.02	.06	−.01
	−.03	.06	−.02	.06	.02	.05	−.01
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.04)
Media consumption	.03	.01	−.08	−.06	−.01	−.05	.04
	.02	.02	−.07	−.06	−.01	−.05	.04
	(.03)	(.04)	(.03)	(.04)	(.03)	(.03)	(.04)
Gender	.02	−.08	.02	−.02	.01	−.03	−.05
	.03	−.12	.03	−.02	.01	−.04	−.08
	(.06)	(.05)	(.06)	(.05)	(.05)	(.05)	(.07)
Class identification	−.07	.04	−.03	−.03	−.02	−.03	.08
	−.04	.03	−.02	−.02	−.01	−.02	.06
	(.03)	(.03)	(.03)	(.03)	(.02)	(.03)	(.04)
Union membership	.07	−.02	−.04	−.03	.03	.02	−.04
	.11	−.03	−.10	−.04	.07	.03	−.08
	(.06)	(.05)	(.10)	(.06)	(.07)	(.06)	(.10)
Intercept	−1.90	−1.39	−1.24	−.94	−1.37	−1.22	−1.60
R^2	.28	.22	.24	.22	.29	.25	.18
N	491	845	489	646	695	684	499
S.e.e.	.59	.70	.63	.62	.61	.63	.72

Note: Entries are, from top to bottom, standardized regression coefficients, unstandardized regression coefficients, and, in parentheses, the standard errors.
[a]This is the weighted total for the EU. Naturally, all coefficients are significant. S.e.e. refers to the standard error of the estimate.
*$p < .01$.
**$p < .001$.

TABLE 5

Continued

MEMBER STATE							
IRELAND	ITALY	LUXEMBOURG	THE NETHERLANDS	NORTHERN IRELAND	PORTUGAL	SPAIN	TOTAL EU
.11*	.05	.13	.08	.12	.07	.05	.12**
.12	.05	.17	.12	.011	.10	.06	.14
(.04)	(.06)	(.08)	(.05)	(.07)	(.07)	(.07)	(.00)
.11*	.03	.03	.14**	−.11	−.05	.05	.08**
.10	.04	.04	.25	−.10	−.06	.11	.10
(.04)	(.08)	(.09)	(.06)	(.08)	(.07)	(.12)	(.00)
.15**	.16*	.01	.21**	.36**	.06	.12	−.23**
.12	.14	.01	.23	.23	.06	.11	−.18
(.04)	(.04)	(.07)	(.04)	(.06)	(.06)	(.05)	(.00)
.09	.13	.12	.17**	.08	.20**	.07	.11**
.05	.09	.09	.12	.05	.16	.04	.07
(.03)	(.04)	(.05)	(.03)	(.05)	(.04)	(.03)	(.00)
.02	.17*	.12	.12*	.11	.20**	.22**	.14**
.01	.11	.07	.09	.07	.13	.14	.09
(.03)	(.03)	(.04)	(.03)	(.05)	(.04)	(.04)	(.00)
.05	.03	.09	.08	.00	.17*	.05	.09**
.03	.02	.06	.06	.00	.10	.03	.05
(.03)	(.03)	(.04)	(.03)	(.05)	(.04)	(.04)	(.00)
.04	−.03	−.05	−.03	.01	.16*	−.02	.00**
.04	−.03	−.05	−.03	.01	.13	−.02	.00
(.04)	(.04)	(.06)	(.04)	(.09)	(.04)	(.05)	(.00)
−.10	−.03	.01	−.05	−.26*	−.05	−.00	−.04**
−.03	−.01	.00	−.02	−.08	−.02	−.00	−.01
(.01)	(.02)	(.03)	(.02)	(.03)	(.02)	(.02)	(.00)
−.03	.01	.03	.03	.06	−.03	.05	.05**
−.01	.00	.01	.01	.01	−.01	.01	.01
(.01)	(.01)	(.02)	(.01)	(.02)	(.01)	(.01)	(.00)
.07	.02	.06	.05	−.02	.06	.03	.04**
.05	.02	.05	.05	−.02	.05	.02	.04
(.03)	(.04)	(.06)	(.04)	(.06)	(.04)	(.04)	(.00)
.01	−.08	−.04	−.00	−.04	−.03	.02	−.01**
.01	−.07	−.04	−.00	−.03	−.02	.02	−.01
(.04)	(.04)	(.07)	(.04)	(.06)	(.04)	(.04)	(.00)
−.04	−.07	−.12	.05	−.01	−.02	−.07	−.01**
−.05	−.10	−.17	.08	−.01	−.03	−.10	−.02
(.06)	(.07)	(.10)	(.06)	(.10)	(.07)	(.08)	(.00)
.04	−.15*	.02	.02	.08	−.01	.03	.02**
.03	−.12	.01	.01	.06	−.01	.02	.01
(.03)	(.04)	(.05)	(.03)	(.06)	(.04)	(.04)	(.00)
.01	.02	.05	.02	−.06	−.08	.00	.01**
.01	.03	.08	.04	−.09	−.18	.00	.02
(.07)	(.09)	(.10)	(.08)	(.12)	(.11)	.15	(.00)
−1.00	−.84	−1.19	−1.50	−.11	−1.42	−1.60	−1.41
.12	.15	.14	.22	.31	.27	.14	.21
591	394	258	673	157	370	382	[a]
.64	.64	.72	.75	.51	.67	.70	.65

measures of support for the rule of law and perceptions of the law as a coercive system (.5 or so in some nations). Political and legal values play a major role in structuring attitudes toward the Court, but the relative impact of the three concepts and indicators remains a little murky.

We find little or no evidence of a connection between ideological self-identification and support

TABLE 6

Reduced Models of Diffuse Support, Attentive Public, 1992

	MEMBER STATE						
VARIABLES	BELGIUM	DENMARK	FRANCE	GERMANY (EAST)	GERMANY (WEST)	G.B.	GREECE
Specific Support for Court of Justice	.07	.23**	.07	.01	.13**	.25**	.11*
	.09	.25	.08	.02	.14	.24	.13
	(.04)	(.04)	(.05)	(.05)	(.04	(.03)	(.05)
Knowledge of Court of Justice	.05	.12**	.10*	.14**	.09*	.08*	.05
	.07	.16	.18	.19	.11	.09	.06
	(.05)	(.04)	(.07)	(.05)	(.04)	(.03)	(.05)
Support for EU	.21**	.22**	.26**	.23**	.24**	.28**	.19**
	.18	.17	.19	.18	.18	.18	.16
	(.03)	(.02)	(.03)	(.03)	(.03)	(.02)	(.04)
Support for liberty	.18**	.11**	.06	.07	.17**	.14**	.05
	.12	.07	.04	.04	.11	.09	.03
	(.03)	(.02)	(.03)	(.02)	(.02)	(.02)	(.03)
Support for rule of law	.17**	.03	.12*	.14**	.21**	.03	.24**
	.11	.02	.07	.09	.13	.02	.16
	(.03)	(.03)	(.03)	(.02)	(.02)	(.02)	(.03)
Alienation from law	.16**	.04	.13*	.19**	.09	.03	.03
	.11	.03	.08	.11	.05	.02	.02
	(.03)	(.02)	(.03)	(.02)	(.02)	(.02)	(.03)
Intercept	−1.39	−1.42	−1.29	−1.37	−1.47	−1.15	−1.18
R^2	.26	.21	.22	.20	.28	.24	.16
N	623	898	579	755	809	784	617
S.e.e.	.59	.70	.63	.62	.62	.63	.72

Note: Entries are, from top to bottom, standardized regression coefficients, unstandardized regression coefficients, and, in parentheses, the standard errors. S.e.e. refers to the standard error of the estimate.

[a] This is the weighted total for the EU. Naturally, all coefficients are significant.

*$p < .01$.

**$p < .001$.

for the Court of Justice. In only one nation, Northern Ireland, does the coefficient reach statistical significance at .01; in the others, we see a mixture of signs. Thus ideological self-identification does not push in one direction across nations. Parties and politicians of the same stripe have taken quite different positions on the EU in different nations, so perhaps our results confirm Lange's observations about the difficulty of saying much about the politics of the EU within particular nations. Our failure to find much is also probably a function of the effects of ideology playing out through other variables in the model. For example, stance on the left-right continuum presumably is connected to some of the other political values in our model. In any event, we see little evidence of consistent national variability in the degree to which the Court is perceived along traditional ideological cleavages.

We return to the Court of Justice itself. To what extent does diffuse support for the Court depend on public satisfaction with specific policies? Earlier we presented modest, sometimes a bit stronger, bivariate correlations between specific and diffuse support for the Court. Do these relationships hold up in the midst of a wide range of controls? In four of the 14 nations, we find some evidence of a connection between specific and diffuse support: it is moderate in Denmark ($\beta = .23$), West Germany ($\beta = .12$), and Britain ($\beta = .24$). Those who are pleased with the Court extend more support to it. There is also something of this relationship in several other countries (e.g., Greece, Ireland, and perhaps Luxembourg and Northern Ireland). In these countries, support is to some degree contingent upon performance, and legitimacy, in consequence, may therefore be quite low. For the rest of the EU (East Germany, Belgium, France, Italy, Portugal, and Spain), we see only a tenuous connection between perceptions of the Court and support; ceteris paribus, those who are satisfied with the current operation of the Court are no more or less likely to be committed to the continued functioning of the institution. The weakness of these relationships is probably not due to those who extend support to the institution despite dissatisfaction with its performance. Instead, the relationships are weak because people do not have enough information to draw a conclusion about the Court's performance, so support depends upon other factors. Thus many people know how they feel about the Court without knowing much about what it has done lately. Satis-

TABLE 6

Continued

MEMBER STATE							
IRELAND	ITALY	LUXEMBOURG	THE NETHERLANDS	NORTHERN IRELAND	PORTUGAL	SPAIN	TOTAL EU
.12**	.06	.14	.08	.18*	.07	.05	.12**
.13	.07	.17	.11	.16	.10	.07	.14
(.04)	(.05)	(.07)	(.05)	(.06)	(.06)	(.06)	(.00)
.12**	.03	.12	.14**	−.10	−.04	.07	.09**
.12	.05	.03	.25	−.09	−.06	.16	.10
(.04)	(.07)	(.08)	(.06)	(.07)	(.06)	(.10)	(.00)
.15**	.16**	.03	.21**	.38**	.12*	.13*	.25**
.12	.15	.03	.24	.25	.13	.12	.19
(.03)	(.01)	(.06)	(.04)	(.05)	(.05)	(.04)	(.00)
.10*	.14**	.13	.20**	.13	.18**	.09	.13**
.07	.10	.09	.13	.08	.15	.06	.08
(.02)	(.03)	(.04)	(.02)	(.04)	(.04)	(.03)	(.00)
.04	.18**	.13	.11*	.09	.20**	.22**	.14**
.02	.11	.08	.08	.06	.13	.14	.09
(.02)	(.03)	(.04)	(.03)	(.05)	(.04)	(.03)	(.00)
.03	.03	.10	.09	−.02	.17**	.06	.09**
.02	.02	.06	.07	−.01	.10	.03	.06
(.02)	(.03)	(.04)	(.03)	(.04)	(.03)	(.03)	(.00)
−1.14	−.89	−1.20	−1.50	−.51	−1.40	−1.52	−1.33
.10	.12	.10	.21	.25	.23	.13	.20
759	512	355	747	183	460	506	a
.64	.64	.72	.75	.58	.68	.69	.66

faction with policy outputs in general, at least in this context, is probably not a good explanation of institutional commitment.

Does awareness of the institution increase diffuse support for the Court? We find slight evidence in favor of this proposition. In two of the nations, East Germany and The Netherlands, knowledge of the Court of Justice reaches statistical significance at .001; in two others, significance is achieved at a somewhat less demanding level. These relationships are never very strong, although we note that the coefficients measure the independent impact of awareness, that is, the impact beyond levels of satisfaction with Court outputs and the other variables in the model. That is a demanding test. Since we are working with a subsample that is by definition aware of the Court of Justice, these findings are not surprising. In a larger theoretical population (those who are aware plus those who are not), the effect of awareness on diffuse support is surely stronger than what we report here.

Nevertheless, the general finding is clear: in most of the nations of the EU, those who are aware of the Court of Justice are no more or less likely than others to show support for the Court. These results hold up in a broad range of specifications of the impact of knowledge. We asked, for example, whether awareness of the Court conditioned the impact of specific support on diffuse support for the Court. Yet for all our efforts, we uncovered little or no evidence for either straightforward or subtle consequences of awareness.

The large number of independent variables, many of which have no substantive impact on support, depresses our coefficients and inflates our standard errors. Accordingly, we have reestimated the equation, dropping the variables with little direct impact on support. We include a variable in this final rendition if it reached statistical significance at .001 in at least two nations. Ultimately, the point at which we should cut is an arbitrary decision; but in this case, there is a clear break between the variables' legal alienation and ideological self-identification. Table 6 reports the results of this reduced model of diffuse support.

The results in Table 6 confirm and strengthen our interpretations of Table 5. Here again the most consistent finding is the connection between support for the EU and diffuse support for the Court of Justice. For 13 of the 14 nations, we observe a strong relationship—often the strongest within a nation—between attitudes toward the Community and the Court. The result is weakest for Portugal, but it is still in the expected direction. It is, of course, reasonable to expect some connection; but perhaps the point to note is strength and consistency of this relationship across the nations of the Community.

This reduced equation improves the performance of the remaining variables across the board. The importance of broad political values, clear enough in our earlier analyses, comes out in even sharper relief. For example, in six of the nations, support for liberty achieves statistical significance at .001 or better, and in one other, at a less stringent level. Awareness of the Court, depressed by other measures of knowledge in Table 5, increases somewhat in importance. In four of the nations, awareness of the Court is associated with higher levels of support. We also see a somewhat larger role for specific support. It is a force in more nations and carries larger coefficients.

Two general points about the results in Table 6 deserve some comment. First, in most countries, this simple, six-variable equation explains a substantial amount of variance in supportive attitudes. Support for the Court is most difficult to explain in Ireland, Luxembourg, Italy, and Spain; but in nearly all of the other countries, we can account for roughly one-quarter of the variance.

Second, although we have sketched the broad outlines of the bases of support for the Court of Justice, few of the conclusions we have reached apply with equal force to each of the countries. Broad political values and support for the Community carry a great deal of weight in virtually all of the nations, but which value looms as most important differs. This heterogeneity justifies our initial decision to focus on nations rather than the Community as a whole. It derives in part from the smaller numbers of cases in Luxembourg and Northern Ireland and in part from substantial national differences in the origins of support for the Court.

We are particularly impressed by Great Britain and Denmark (and to a lesser degree Northern Ireland). The strong coefficients for specific support in Denmark and Great Britain are unusual, as is the relatively weaker role of values in evaluations of the Court. We suspect that some sort of complex interaction is occurring here, probably based on the sort of information that has permeated the society. In some of the nations of the EU, the Court has made quite controversial and visible decisions; in others, the Court is, for the most part, invisible or associated with routine, noncontroversial issues. Thus the meaning of awareness of the Court differs across nations, depending upon the nature of the information available to the public. In the absence of political and legal information, then, people rely heavily on general political values to make up their minds about the Court of Justice.

CONCLUSION

What have we discovered from our initial foray into the etiology of support for the Court of Justice? If, for a moment, we free ourselves from the constraints of any particular coefficient, we see some broad patterns in this analysis. Our findings indicate that attitudes toward the Court are shaped in part by attitudes toward the EU. Without a doubt, mass publics in Europe see the Court in substantial part as an institution of the Union. Yet attitudes toward the Court are shaped by more than how one feels about the EU.

We have seen bits of evidence suggesting that those who are aware of the Court of Justice tend to support it more. We have interpreted this as evidence that the dominant messages one receives in some nations about institutions like the Court are positive. This is not surprising; cognitive mobilization and social learning typically contribute to system maintenance and stability. There are surely some who have considered and decided against the Court, but in the more common pattern, those who have not developed support for the institution are simply not engaged enough to have acquired the requisite information.

This is especially likely in view of the current nonideological image of the Court among the mass publics of Europe; that is, at this point, citizens do not see the Court as a leftist or rightist institution; nor is it viewed as supportive of particular group interests (see Weiler 1994). Thus before the Court acquires an ideological identity, it probably has the ability to rely on traditional judicial symbols to communicate with its various constituencies.

We have discovered an important role for basic political values in the creation of support for the Court. Perhaps this is only the case for institutions of relatively low visibility, but many people seem to base their views of the Court on their underlying values toward law and liberty. This result suggests that popular legal cultures may play an important role in the functioning and maintenance of judicial institutions. Accordingly, how people come to feel about law has implications for their stances toward the courts and other legal institutions. In light of their possible importance, it is a pity that we know so little about the origins of cultural orientations toward law.

We should return as well to our basic findings about the legitimacy of the Court. We have not found a great deal of diffuse support for the Court. Of course, we had good reason to expect such a pattern for an institution that has only recently achieved any public visibility at all. And from the point-of-view of the Court, it is encouraging that dissatisfaction with the operation of the institution is practically nil. We suspect that we are observing a court that may be on the way to developing diffuse support. That is, it may be gaining visibility while maintaining some level of specific support. If the Court continues to meet the expectations of the citizens of the EU, then this unawareness may be converted to specific support; and specific support may be transformed into diffuse support.

The basic political strategy of the Court of Justice over the last several decades has been one of pursuing anonymity as a means of avoiding opposition to its pioneering Community-building decisions. That may have worked in the early days of the Community. But, today, with the growing popular awareness of the EU and the increasing concern over the Community's democratic deficit, the Court can no longer maintain low visibility. Moreover, it seems inevitable that the Court will be making decisions on cases of wide interest to the mass public. For the Court, our findings may well imply that an aggressive campaign to associate itself with fundamental values such as liberty and the rule of law could have important benefits; for scholars, our findings point to the necessity of developing a longitudinal study of the evolution of opinions toward the Court. Longitudinal analysis would also have the happy benefit of permitting us to resolve many questions about the impact of the timing of our survey during a period of high controversy over the EU.

APPENDIX

Measurement of Variables

Specific Support for Court of Justice. "Has what you heard [about the European Court of Justice] or read given you a generally favorable or unfavorable impression of the European Court of Justice?" Generally favorable, generally unfavorable, neither favorable nor unfavorable, don't know.

Awareness of Court of Justice. "Let us now turn to another institution of the European Community: The European Court of Justice. This court sits in Luxembourg and is the highest court of the European Community as a whole. Would you say you are very aware, somewhat aware, not very aware or have you heard of the European Court of Justice?"

Support for European Union. This index has five items:

1. "Generally speaking, do you think that (OUR COUNTRY'S) membership of the European Community is a good thing, a bad thing, neither good nor bad?"
2. "In general, are you for or against efforts being made to unify Western Europe? For—very much, For—to some extent, Against—to some extent, Against—very much."
3. "Personally, would you say that the Single European Market which will come about by the end of 1992, the beginning of 1993 makes you feel very hopeful, rather hopeful, rather fearful or very fearful?"
4. "Overall, do you think that the completion of the Single European Market in the end of 1992, the beginning of 1993 will be for people like you a good thing, a bad thing, or neither a good nor a bad thing?"
5. "If you were told tomorrow that the European Community had been scrapped, would you be very sorry about it, indifferent or very relieved?"

A single, very strong factor emerged from the common factor analysis, accounting for 61.1% of the variance (eigenvalue = 3.07; for the second factor, the explained variance is 15.1 and eigenvalue = .75).

Support for Liberty. This is a two-item scale. The respondents were asked to evaluate the following statements using a five-point Likert response set: (1) "Society shouldn't have to put up with those who have political ideas that are extremely different from the majority" and (2) "It is better to live in an orderly society than to allow people so much freedom that they can become disruptive." The index we employ is simply the mean of the two responses.

Support for Rule of Law. This measure includes two items: (1) "It is not necessary to obey a law you consider unjust" and (2) "Sometimes it might be better to ignore the law and solve problems immediately rather than wait for a legal solution." One who disagrees with these statements is exhibiting a strong commitment to the rule of law. Here again this index is the mean of the two responses.

Alienation from Law. The items for this measure include (1) "It is rare that the law is on my side; usually, I find laws to be restrictive and against my interests" and (2) "My interests are rarely represented in the law; usually law reflects the views of those who want to control me." This index is also the mean of the two responses.
Democratic Deficit. "Do you think citizens have sufficient democratic influence in EC decision-making or not?" Yes, no, don't know.

Ideological Self-Identification. "In political matters people talk of the 'left' and the 'right'. How would you place your views on this scale?" This scale runs from 1, the furthest left, to 10, the furthest right.

Educational Attainment. "How old were you when you stopped full-time education? Since stopping full-time education, have you (1) resumed general education at all, (2) taken an apprenticeship or professional training for your job?"

Opinion Leadership. This is a standard Eurobarometer index based on two items: (1) "When you hold a strong opinion, do you ever find yourself persuading your friends, relatives, or fellow workers to share your views? Does this happen often, from time to time, rarely, never?" and (2) "When you get together with friends, would you say you discuss political matters frequently, occasionally, or never?" In each case, respondents had the option of saying don't know.

Media Consumption. "How often do you . . . (a) watch the news on television? (b) read the news in daily papers? (c) listen to the news on the radio?" The options were everyday, several times a week, once or twice a week, less often, never, or don't know.

Gender. Male or female.

Class Self-Identification. "If you were asked to choose one of these five names for your social class, which would you say you belong to?" Legitimate responses included middle class, lower middle class, working class, upper class, upper middle class, and refuses to be classified (the last of which is missing data).

Trade Union Membership. "Are you yourself a member of a trade union?" Yes, no.

Salience of Community. "Have you recently heard or read something about the European Community (the EC) or the European Economic Community (the EEC), its policies, its institutions?"

Salience of Commission. "Have you recently seen or heard, in the papers, on the radio, or on television, anything about the European Commission in Brussels, that is, the Commission of the European Communities?"

Salience of Parliament. "Have you recently seen or heard, in the papers, on the radio, or on television, anything about the European Parliament, that is the parliamentary assembly of the European Community?"

Salience of Treaty. "Have you recently heard anything about the Treaty of Maastricht? This Treaty foresees, within the European Community, the development of a European Union with a single currency and a common foreign and security policy?"

Salience of Court of Justice. "Let us now turn to another institution of the European Community: the European Court of Justice. This Court sits in Luxembourg and is the highest court of law for the European Community as a whole. Have you recently heard or read about the European Court of Justice?"

Salience of (YOUR COUNTRY'S) *High Court.* "And what about the (HIGHEST COURT OF YOUR COUNTRY), that is one of the high (NATIONALITY) courts? Have you recently heard or read anything about it?"

Awareness of Court of Justice (Repeat). "Now once again on the European Court of Justice, would you say you are very aware, somewhat aware, not very aware, or have you never heard of the European Court of Justice before today?"

Institutional Commitment. "If the European Court of Justice started making a lot of decisions that most people disagree with, it might be better to do away with the Court altogether."

Institutional Powers. "The right of the European Court of Justice to decide certain types of controversial cases should be reduced."

Judicial Independence. "The political independence of the European Court of Justice is essential. Therefore, no other European institution should be able to override court opinions even if it thinks they are harmful to the European Community."

Notes

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1. The Court of Justice serves as the highest court for the various European Communities, including the EU. Thus, although the Community has now become the EU, the proper term continues to be the Court of Justice of the European Communities. We executed our survey of mass publics in late 1992, several months prior to the official beginning of the EU; but in order to avoid confusion, we use the term EU except when we make historical references.

2. Weiler (1990, 1991) provides a good review of the literature on the deficit of democracy. The term *democratic deficit* refers to the lack of means by which citizens of Europe can control the governmental institutions of the EU, at least in comparison to the typical situation in the parliamentary democracies of Western Europe. More precisely, the "democratic deficit in the Community can be defined as 'the combination of two phenomena: (i) the transfer of powers from the Member States to the EC; (ii) the exercise of these powers at Community level by institutions other than the European Parliament, even though, before the transfer, the national parliaments held power to pass laws in the area concerned" (Featherstone 1994, 150; see also Boyce 1993). Despite some changes in the direction of greater popular control, the EU still has few of the political levers the citizens of national states take for granted in their domestic politics. And the concept of a democratic deficit is a matter of both theoretical and practical consequence: in a recent poll of the publics of the EU, Eurobarometer 38.0 (September–October 1992), more than 75% expressed concern about the lack of democratic institutions in the Union.

3. The many studies of presidential popularity, for example, tap into specific support for the executive and executive branch (e.g., MacKuen, Erikson, and Stimson 1992), and most but not all research on public evaluations of Congress deals with specific rather than diffuse support (see Patterson and Caldeira 1990). Students of support for European integration do not distinguish between diffuse and specific support, but in practice, they typically focus on items we think pick up on more transient evaluations (e.g., "Do you think your that your country's membership in the European Union is a good thing, neither good nor bad, or a bad thing?"). For recent work, see Gabel 1994.

4. We do not have the space here to review specific decisions or even bodies of law to provide evidence for the aggressive activism of the Court of Justice. For reviews of and commentary on the jurisprudence of the Court of Justice, see, in general, Browne (1987), Lasok and Bridge (1991), and Shapiro (1993).

5. Awareness of the Court will not invariably produce support; but if knowledge or awareness is acquired through exposure to disproportionately positive messages (as we think it is), then generally higher levels of support are likely to occur (see, in general, Zaller 1992). We hypothesize, accordingly, that the dominant messages propagated about the Court of Justice are positive and supportive and thus that cognitive mobilization is associated with diffuse support.

We do not have systematic evidence on whether political elites portray the Court in a favorable light; but in our review of a selection of national newspapers, we found little or no critical commentary from political leaders or other elites. We wanted to obtain a sense of how often and in what light the mass media across the EU portrayed the Court of Justice, but we lacked the massive resources required to do this in a comprehensive and systematic fashion. Instead, we chose a leading newspaper in each of France, Spain, Ireland, Italy, Germany, and the United Kingdom and did a count of the number of stories on the Court of Justice over the past five years. In this survey, we encountered very little of a critical nature.

Law professors, especially professors of European law, have been among the chief proponents of the Court of Justice, propagandizing in favor of it in many different forums (see, esp., Shapiro 1992). It is difficult to find critical commentary on the Court of Justice in European law journals. This, of course, is the standard fare on the Supreme Court in American journals.

We can think of one important exception to the general rule of elite support for the European Court of Justice. For the last two or three years, Chancellor Kohl of Germany offered critiques, some of them scathing, of the European Court of Justice's decisions on a number of issues (see "Die Leise Ubermacht," *Der Spiegel*, October 1992, p. 102). Indeed, some commentators attribute the court-stripping provisions of the Maastricht Treaty to Chancellor Kohl and his allies.

6. We have followed the weighting strategy recommended by the Eurobarometer. For the data from individual member states, this entails little more than minor "poststratification" to correct for systematic deficiencies in the response rates. When we report a total for the EU, however, we also weight the data by the population of the country, thus providing a Union-wide estimate corrected for the similarity of the size of sample in most countries despite the different sizes of the populations. The data are currently available in ICPSR study 6044.

7. We do not necessarily have high expectations of the substantive content of the awareness of the Court. No doubt many respondents would find it impossible to report very much information about the Court. We suspect this is also true of other Community institutions, and of most domestic political institutions as well.

8. The substantial proportions of respondents who lack information about the Court of Justice and the variability of this number across nations raise potential problems in the proper estimation of our models of diffuse support. For many of our respondents, we have complete data on independent variables but no observation on the dependent variable,

diffuse support. Thus, the dependent variable is *censored*. Nonrandom censoring can make problematic the use of standard techniques: "If the rule by which units are censored . . . is independent of the dependent variable, standard techniques may be used without problem" (King 1989, 208). If censoring is not random, ordinary least squares will produce biased, inconsistent, and inefficient estimates. Econometricians have developed a number of models to deal with problem of "selection bias" (see Maddala 1983, 257–90). We have used Heckman's two-stage model to test for the presence and severity of this problem and to correct it where required (Greene 1993). It involves two equations/models: one of information holding about the Court of Justice (present or absent) and one of support for the Court. We have not strived for a comprehensive model of information holding but, rather, one good enough for the econometric purposes at hand. For most of the nations, the correction makes no difference in the coefficients and standard errors. In a couple of cases, we observe small differences, none of which would change our interpretations of ordinary least squares. So, in the absence of signs of severe "selection bias," we have opted for presenting the results of ordinary least squares.

9. The factor loadings and communalities are, respectively, .64 and .41 for institutional commitment, .54 and .30 for institutional powers, and .22 and .05 for judicial independence. For the first dimension, the eigenvalue is 1.43, for the second, .92. The reliability of the scale is fairly low. Cronbach's alpha for the three-item scale is .42, largely due to difficulties in the third item and, of course, the relatively small number of measures employed.

10. Do responses to the item reflect attitudes toward "other European institutions" that might override the Court's decisions? To test this notion, we regressed the responses to item 3 on item 1 (the best measure of diffuse support) and a conventional measure of support for a united Europe. These two variables account for a small portion of the variance in item 3 ($R^2 = .03$). The standardized regression coefficient for the first support item is .13; for attitudes toward a unified Europe, the coefficient is .10. Thus, if anything, the variance in the item is more closely associated with diffuse support than with support for European unification. We conclude that something else, not systematic error, makes this item less useful.

11. For those who had not heard or read anything about the Court, we asked: "Generally, do you have a favorable or unfavorable impression of the European Court of Justice?"

12. We define the categories in terms of the standard deviation of the factor score, as follows: *low* (<.5 standard deviation below the mean), *moderate* (−.5 to .5 standard deviation around the mean), and *high* (>.5 standard deviation above the mean).

13. We report standardized and unstandardized coefficients and standard errors. Generally we accept as substantively significant an unstandardized coefficient approximately three times its standard error. These are randomly selected samples, so we also report significance tests, although the large number of cases involved in some of the analysis requires that we impose a .001 probability level not to reject our null hypotheses. We relax this standard for the two countries with fewer respondents (Northern Ireland and Luxembourg).

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THE PRIMACY OF LABOR IN AMERICAN CONSTITUTIONAL DEVELOPMENT

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As demonstrated in landmark decisions of the U.S. Supreme Court, American constitutional development has been fueled since the framing by disputes arising from changing labor relations in both private and public settings. This pattern is explained by the original provisions of the Constitution, the English background of its emergence, and the primacy of labor *as a theoretical concept for studying political change. The Court's decisions protecting property express the status quo, the establishment against which transformations proceed. However, the property cases may also be reinterpreted along the lines indicated.*

Professional study of the U.S. Constitution is occupied by an attention to the document's formal characteristics and their change over time: separation of powers, individual rights, judicial review and its theoretical justifications, the determinacy or indeterminacy of legal rules. To the extent that this study has a social theme, it is most often the relationship of the constitutional order to the institution of private property—how public law has promulgated, promoted, and protected, in the short and long run, the interests of various kinds of private property holders.[1] Whereas property is thought to be related organically to the Constitution through the motives and worldview of the framers, other social subjects are treated as constitutionally adventitious, historically particular, their position in the broader development of fundamental law left unspecified.

Consider, for example, *Dred Scott* v. *Sandford* (1857). *Dred Scott* is seen as an expansion of judicial review, declaring the Court's authority to contravene national legislation, for the first time not on an issue concerning the governance of courts but on the most pressing political question of the day. The decision also is understood to express the Jacksonian judiciary's policy of confining Congress' sway over the states. Finally, the case is taken to illustrate the Constitution's protection of vested rights, especially the rights of property. In all American history, *Dred Scott* has furnished perhaps the most egregious example of the political system's deference to property over competing values (Fehrenbacher 1981; Meister 1989).

Not so much unnoticed as unattended analytically is the fact that *Dred Scott* was about the survival of a labor system upon which at least half the states in the federal compact were economically dependent. That slavery was a species of labor is obvious in any historical consideration of the case; but the deeper implications of this commonplace for constitutional development remain obscure. Absent an analytic link between the formal, constitutional implications of the Court's decision and the question of forced labor that occasioned the lawsuit, the association of constitutional and social issues appears fortuitous, the results—supporting states rights against Congressional power—as likely to have been produced by some other question as by slavery. Similarly, a regard for slavery as an institution with important constitutional ramifications—but not as a species of labor—has led to the slave's being regarded as a historical anomaly, inadequately conceived as property, cut off from a comprehensive examination of changing state-societal relations.

I take a contrary approach, arguing that American constitutional development has been inextricably connected to questions of labor or work relations; that changes in labor relations, as distinguished from property relations, have, since the founding of the republic, driven major constitutional change; and that American constitutional development has required, above all, the inclusion of society's essential work tasks within core principles of liberalism.[2] This is not to deny that protection of property was a foremost objective of the framers, that property has been a continuous subject of constitutional adjudication, or that property is related, conceptually and historically, to questions of labor. However, property is a background, an establishment against which other changes proceed and, for that reason, a relatively poor guide to constitutional turning points.

As a strategy of presentation, in order to test the argument against prevailing ideas, I avoid introducing case materials not already well known on the constitutional record. The aim is not to unearth new decisions or to reinterpret the results of the decisions discussed except as they are proposed here as an alternative constitutional "canon," responsive to a different dynamic of political development. The essay would miss its mark, however, if it were mainly contrarian. I also advance, in a preliminary way, both historical and theoretical explanations for why labor questions should have had the transformative effects they did.

First I offer a brief chronology of landmark Supreme Court cases in which labor issues underlay the constitutional questions decided. I make no claims for why labor should have figured in these decisions or that labor issues were necessary to the results, my purpose being to establish a prima facie case for labor's constitutional salience. I then place this chronology against the constitutional document as originally framed and within the longer sequence of Anglo-American constitutionalism. These steps are necessary to show the institutional arrangements that changing labor relations in the United States would unsettle. I also argue for the *primacy of labor* as a theoretical concept in studying political development. In this perspective, the constitutional pattern at hand illustrates government's fundamental dependence on work relations, as well as the role of state structures in their regulation.[3] Finally, I support the proposition of labor's primacy by assessing the more traveled interpretive route of property and indicating how it might be remapped along the lines suggested.

THE WORK OF THE CONSTITUTION

A review of landmark decisions by the Supreme Court over American constitutional history reveals an unexpectedly large number that have arisen in disputes concerning work or labor relations. By landmark decisions I have in mind those commonly acknowledged to have defined the framework of government or the connections of state to society, decisions that could be expected to appear on a standard boiler-plate syllabus in Con Law. Being constitutional, they not only pertain to the parties before the bar, or even to groups or interests the parties may represent, but announce parameters for future public and private action generally. By labor relations I refer to relations between employer and employee, master and servant (or slave), supervisor and subordinate, typically (though not always) involving compensation and the possibility of discharge, as these occur at all levels of work organization and in both public and private settings.

Moreover, if one gleans from the mere landmark decisions those that are constitutional turning points —that is, those that do not merely refine or extend already established principles but mark a shift in authority among government institutions and/or between state and society—Court decisions based on labor facts become even more prominent. It is possible briefly to identify several.[4]

1. *A labor issue provided the foundational case for the constitutional practice of judicial review.*

Chief Justice Marshall's decision in *Marbury* v. *Madison* (1803) marked a turning point in the theory and practice of the judiciary within the framework of the American constitutional system. In *Marbury*, the Supreme Court for the first time held an act of Congress—Section 13 of the Judiciary Act of 1789—invalid. In his opinion, Marshall asserted that it was the judges' duty to "say what the law is," and he initiated the method of interpreting the text of the Constitution in the manner of an ordinary statute. This was a departure from the previous understanding of judicial power as protecting the limits of government against plainly unconstitutional violations, like the denials of jury trial and uncompensated seizures of property that had occurred before the Constitution's adoption (Snowiss 1990).

The *Marbury* case concerned the new and vexing problem of staffing a democratic government, with William Marbury taking his place at the head of a long line of disappointed office seekers in American political history. The commission Marbury sought was as a justice of the peace for the District of Columbia, a position compensated by fees and valued as an entry point to higher office. Perhaps because he had no interest in fostering Federalist cadres, Jefferson reduced the number of Adams' appointees, eliminating Marbury. In the pattern of Anglo-American office seekers for centuries, Marbury argued his case according to the common law rights attached to his appointment, which was for a fixed tenure of five years; it was largely on that basis that Marshall endorsed Marbury's entitlement to relief (Orren 1994).

The *Marbury* case engaged two other work-relations issues. Marbury sued for the common law writ of *mandamus*, directing the secretary of state to deliver his commission. Although attention historically has focused on Marshall's assertion of the judiciary's power over the Congress, Republicans of the period and Jefferson himself considered the main provocation to be the Court's invasion of the president's authority over his subordinates in the executive branch. The Court's deliberations over Marbury took place in the shadow of Congress's removal of 16 circuit court judges through the repeal of the Judiciary Act of 1801 that had authorized their appointment (O'Fallon 1992). Congress had delayed the Court's session for an entire year, during which the impeachment of other Federalist judges, Marshall among them, was openly discussed.

The only other time before the Civil War that the Supreme Court invalidated an act of Congress was in *Dred Scott* v. *Sandford*, declaring the Missouri Compromise unconstitutional and former slaves ineligible to become citizens. The labor content of *Dred Scott* was not limited to the institution of Southern slavery. Antislavery men like those who financed and argued Scott's cause believed the stakes involved the dignity of all labor and the civil liberty that free labor epitomized (Foner 1970). By the Court's decision, however, labor as property was for the time being preserved, along with constitutional arrangements arrayed in its defense.

2. *Labor issues were at the heart of the judiciary's expansive assertion of authority under the Fourteenth Amendment, with a labor case lending its name to the so-called "Lochner Era" in American constitutional history.*

The half-century beginning in the 1880s has been called the era of "laissez-faire constitutionalism" because of the regular overturning by state and federal judges of legislation curbing diverse practices by business. Accomplished under the doctrine of substantive due process, this activity reversed a pattern of judicial deference to legislatures that had prevailed before then. The legislation did not only concern relations with employees; indeed, it was the reversal of laws regulating commerce pure and simple that characterized the judicial about-face, since few significant labor laws had been passed prior to the late nineteenth century. On the other hand, evidence from the case materials of the period supports the conclusion that labor reform was more likely to draw the judges' fire than the reining in of business in other respects.[5]

Within the doctrine of substantive due process, "liberty of contract" came closest to denying the validity of legislation per se. The line of liberty-of-contract opinions leads from the initial reference in dicta to "undue interference with mens' rights of making contracts" in an Illinois opinion on a statute regulating the calculation of miners' wages;[6] to the first state court decision to invalidate a statute as an unconstitutional infringement on liberty of contract, a Pennsylvania statute requiring iron mills to pay wages in cash;[7] to the first U.S. Supreme Court decision to invalidate a state statute based on liberty of contract, *Lochner* v. *New York* (1905), overturning a limit on the working hours of bakery employees (Pound 1908).

During this era, disputes over the constitutionality of purely commercial regulations were often argued *as if* they concerned employees. For example, in the *Slaughter-House Cases* (1873), the first Supreme Court decision interpreting the Fourteenth Amendment, plaintiff's counsel and the four dissenting justices argued that Louisiana's grant of a monopoly for slaughtering beef to a corporation interfered with the pursuit of common trades; Justice Field's dissent verged on claiming that New Orleans butchers had been forced into involuntary servitude, forbidden by the Thirteenth Amendment (1873, 90–91).[8] Similarly, in *Munn* v. *Illinois*, dissenters likened plaintiff warehouse owners to shoemakers and tailors (1877, 138).

3. *An issue of work relations occasioned the determination of plenary authority in the president under the "executive power" provided in Article II.*

By its statutory interpretations, the Court nibbled around the edges of the "new administrative state" installed during the Progressive Era. Constitutional arrangements remained settled, as the majority of justices gave Congress a wide berth to regulate businesses ranging from railroads to meatpacking.[9] A departure from this pattern was *Myers* v. *United States*. *Myers* held for the first time that Article II, Section 1, "grants to the President the executive power of the Government," rather than merely designates his office (1926, 162). As a result, Congress could not encumber the president's authority to remove other officers in the executive branch. Before this, plenary executive power had been upheld outside of wartime only in circumstances when "the peace of the United States" was considered at risk.[10]

Myers, a postmaster dismissed by President Wilson and now deceased, sought (through his widow) to recover back-salary due for his full appointed term, based on the grounds that the president had violated a 1876 statute requiring that postmasters' removals be approved by the Senate. The Court, however, held that both that statute and the Tenure of Office Act of 1867, which imposed this requirement for all civil officers appointed with senatorial consent, were unconstitutional. Justice Taft's opinion said that the president, in order to fulfill his duty faithfully to execute the laws, must possess the "disciplinary influence" of independent removal power over his subordinates (p. 132).

Following a suggestion in *Myers* that the chief executive might independently remove officers with quasi-judicial powers, President Roosevelt fired a Republican-appointed federal trade commissioner, William E. Humphrey. When Humphrey contested the dismissal by suing for his salary, the Court, in *Humphrey's Executor* v. *United States* (1935), denied the president's removal power over agencies not wholly executive in their functions. This turn of events halted Roosevelt's attempt at comprehensive administrative reform. Both decisions remain good law today, allocating authority among the branches.

4. *A labor case signaled the Supreme Court's final abandonment of common law ordering of personal relations and its deference to the legislative branch on matters of social policy.*

National Labor Relations Board v. *Jones & Laughlin Steel* (1937), upholding the Wagner Act of 1935, is the cardinal decision of modern employment law. Exhibiting "the switch in time that saved nine," its impact went to the respective powers of the Court and Congress and to the legal rules governing civil society. The Court's holding—that Congress might enact national measures to regulate private industrial disputes in a constitutional exercise of its powers under the interstate commerce clause—has had repercussions on virtually every policy area, from agricultural production quotas to civil rights.[11] Chief Justice Hughes denied that the National Labor Relations Board proceeding in question had deprived the company of its rights at common law. The proceeding, Hughes said, was "unknown to the common law": it was a "legislative proceeding." Thus, he removed an obstacle that had thwarted the Congress's will in previous labor statutes, the requirement that legislation must conform to preexisting common law rights as interpreted by judges.[12]

Labor's position in this constitutional turning point was not a fluke: the decision that had blocked the New Deal's recovery program, *Schechter Poultry* v. *United States* (1935), was also a labor case. Although known as the "sick chicken" decision because selling unhealthy birds in violation of the National Recovery Administration's live poultry code was an offense alleged, it stated unequivocally that "the question of chief importance relates to the provisions of the Code as to the hours and wages of those employed in defendants' slaughterhouse markets" (p. 548). The actual "switch in time"—Justice Roberts' move to the pro–New Deal position on the Court—had taken place in *West Coast Hotels* v. *Parrish* (1937), upholding

Oregon's minimum wage law for women. Other labor cases tied up constitutional loose ends of the new legislative regime. *Opp Cotton Mills* v. *Administrator* (1941), arising under the Fair Labor Standards Act (FSLA), turned aside *Schechter Poultry*'s suggestion that Congress must provide detailed guidelines when it delegated authority. *United States* v. *Darby* (1941), also upholding the FSLA, found that the legislature might judge for itself the legitimacy of means it devised to regulate commerce. *United States* v. *Hutcheson* (1941), applying the Norris–LaGuardia Act, rejected the parsing of separate legislation in favor of statutory interpretation within broad bodies of law.

5. *A labor issue underlay the shift of power from the judicial to the legislative branch in the essential matter of amending the Constitution.*

Coleman v. *Miller* (1939) held that the validity of state ratifications of constitutional amendments was a "political question," properly determined by the Congress. In so far as amendments already had the sanction (or, for that matter, the opposition) of the Congress, Congress's new authority to decide which amending processes were acceptable further increased the power of the national legislature over the legislatures of the states.

The amendment at issue would have empowered the Congress "to limit, regulate, and prohibit the labor of persons under eighteen years of age." This was a proposal to invade a field of regulation that traditionally belonged to the states and one that the Court before 1937 had specifically designated as outside the ambit of the interstate commerce clause.[13]

6. *A labor case defined for the post–World War II period the limits of presidential authority under nonstatutory claims of national emergency.*

Circumstances after the 1945 armistice—the overseas stationing of troops, nuclear weapons, and other aspects of the Cold War—created a spillover from military to domestic affairs and a de facto expansion of executive power. In 1952, during the U.S. military engagement in Korea, President Truman authorized the secretary of commerce to seize and operate steel mills, on the reasoning that a threatened strike in that industry would impair the national defense. The steel companies obtained an injunction, sustained by the Supreme Court in *Youngstown Sheet & Tube* v. *Sawyer*. Justice Black, writing for the Court, held that only Congress, as the Constitution's designated lawmaker, could authorize the president to take such action, and Congress had not done so (1952, 587). All six members of the majority wrote opinions, but they clearly establish that presidential subordinates will be objects of injunctive relief upon pleas that their actions are illegal.

Precedent for nonstatutory exercise of presidential authority had been established in *In re Neagle* (1890), concerning the employment of a federal marshal, and *In re Debs*, arising from the use of federal troops to quell the Pullman strikers. Chief Justice Vinson cited both in his *Youngstown Sheet & Tube* dissent (1952, 687–88, 702). That in circumstances of emergency Congress might validly pass legislation that would otherwise be unconstitutional had been decided in the setting of a labor dispute, a threatened national railroad strike on the eve of World War I. President Wilson had successfully pressed for the Adamson Act, providing for an eight-hour-day for railroad employees and establishing a commission temporarily to fix wages. The Court, in *Wilson* v. *New* (1917), upheld the act as a provisional measure to avert national calamity.

7. *The hallmark decision of the Warren Court's "countermajoritarian" jurisprudence concerned the absorption into effective national citizenship of persons descended from the labor system of slavery.*

Various constitutional and statutory gestures to redress the aftermath of America's "peculiar institution" had gone unenforced since Reconstruction. With *Brown* v. *Board of Education* (1954), the Supreme Court embarked on a corrective course, for two decades outpacing the Congress with antidiscrimination remedies across a broad institutional front. To be sure, *Brown* concerned issues other than labor: the ideology of race subordination had always extended beyond the servitude it outlived. That occupational parity of black and white, however, was a chief aim of school integration is evident both in the history of the litigation and words of the Court's decision. The "intangible factors" at the cornerstone of *Brown*'s reasoning (p. 493) had been laid by the Court's previous agreement that black lawyers were hampered in their work by segregated professional education.[14]

In the mid-1980s, lingering constitutional tensions converged on unequal employment. The Rehnquist Court's reversal of direction, and the Congress's testing of the justices' deference to legislative majorities by a new civil rights statute in 1991, came to a head as a result of a series of adverse decisions concerning the alleged hiring and stratification of employees by race and the use of affirmative action to remedy past discrimination.[15]

8. *A labor statute was the occasion for the Court's first reversal since the New Deal of a social policy of the Congress, based on the commerce clause.*

After 1937, Congress seemed to exercise unbounded authority through its power over interstate commerce. In 1976, however, in *National League of Cities* v. *Usery*, the Burger Court revived the doctrine of dual federalism, finding a "state sovereignty limitation" in the Tenth Amendment, which provides that powers not delegated to the Congress are reserved to the states, to be applied "in areas of traditional governmental functions" (p. 852). Upon that basis, the Court held that amendments to the FLSA that extended wage-and-hours standards to state and local government employees were unconstitutional.

The uncertainty of this retrenchment was apparent in a second labor case, in 1985. In *Garcia* v. *San*

Antonio Metropolitan Transit Authority, the Court took the unusual step of expressly reversing its earlier decision. Following *National League of Cities*, the San Antonio Mass Transit District had announced it would no longer pay its employees overtime. The Court, in the meantime, had ruled that state-owned commuter railroads did not qualify for immunity against the FLSA because owning railroads was not a "traditional government function."[16] In *Garcia*, this test was rejected altogether, on grounds that it led "to inconsistent results at the same time that it disserves principles of democratic self-governance" (p. 546). Instead of the Tenth Amendment, the Court said, states must rely on the "built-in restraints" of the political process (p. 556).

9. *The Court's turn to "modern substantive due process" implements the decline of the family as a system of obligatory labor.*

The decisions on state regulation of reproductive rights, beginning with *Griswold* v. *Connecticut* (1965) and including *Roe* v. *Wade* (1973) registered the abandonment of the enforceable duty of labor, including childbearing and child rearing, traditionally imposed on women (and sometimes other family members) by ancient common law. The liberty interest protected in these cases, while initially described as "privacy," has more recently come to be expressed in terms of women's work. *Planned Parenthood* v. *Casey*, reviewing Pennsylvania's antiabortion statute, acknowledges the "special responsibilities" of home and family that precluded women's full and independent legal status under the Constitution (1992, 2831). Justice Blackmun, concurring, goes so far as to find a work relation between woman and state when he characterizes restrictions on abortion:

> "By restricting the right to terminate pregnancies, the State conscripts women's bodies into its service, forcing women to continue their pregnancies, suffer the pains of childbirth, and in most instances, provide years of maternal care. The State does not compensate women for their services; instead it assumes that they owe this duty as a matter of course" (p. 2847).

In its rejection of common law ordering, the "activism" of the post-Warren Court in *Roe* is united with the "deference" of the New Deal Court in *Jones & Laughlin Steel*. As in *Brown*, the constitutional doctrine contemplates a change in the employment prospects of citizens most directly affected. In *Casey*, Justice O'Connor's majority opinion supports the adherence to *Roe* by the fact that the "ability to participate equally in the economic and social life of the Nation has been facilitated by [women's] ability to control their reproductive life" (p. 2809). It should be emphasized that "modern substantive due process" in this reading does not present an analogy *to* labor but *labor proper*, albeit performed in a family setting, without compensation in the usual sense, and for which nonfamily persons are often hired without affectionate ties attached.[17]

LABOR'S DEVELOPMENTAL PRIMACY

How might one account for this series, the repeated association of constitutional turning points with labor relations? There are three answers, each instructive on labor's developmental impact. The first is (mainly) *institutional*, focusing on the provisions built into the constitutional design: the decisions in question reconfigured some arrangement of the Constitution that either as originally written or subsequently interpreted presumed an arrangement of labor relations changed or brought under scrutiny by the facts before the Court.

Looking no further than the Philadelphia convention of 1787, it is clear that labor—free and unfree, public and private—was woven into the constitutional fabric in diverse ways. Most familiar here are the framers' accommodations of the two labor systems, South and North: the fugitive slave clause, the continuation of the foreign slave trade, the prohibition of export taxes, the federal number of three-fifths, equal representation in the Senate. Just as significant, however, were ostensibly nonlabor provisions that incorporated assumptions about the regulation of work the framers would have taken for granted. In the eighteenth century, for example, a division between national and state authority dictated state jurisdiction over those departments of law in which English suits were brought in local courts, including land law, criminal law, health and safety, and the law of domestic relations—the category that encompassed relations between master and servant (and slave), husband and wife and parent and child.

In addition, beneath the more exalted purposes of separating powers, the Constitution was an elaborate job description for government personnel. The new government required staffing, by higher as well as lower functionaries. The founding document engaged all of the regular incidents of employment—how positions would be filled, tenure, how salaries were fixed, the process and causes of discharge. A full century before civil service reform, it created offices that reflected the English experience of balancing subordination and autonomy, employment and property. Details deferred were quick to be disputed: the president's power to remove subordinate officers was the subject of the first major constitutional debate in the House of Representatives.

Finally, the Constitution assumed an ordering of labor relations in which male persons who depended on either public or private employment for their livelihoods represented only a small part of the nonslave population. Some framers imagined the dangers of the opposite circumstance. Gouverneur Morris, for example, sought to impose a property qualification for suffrage on the states; the country would soon "abound with mechanics and manufacturers who will receive their bread from their employers. Will such men be the secure & faithful Guardians of liberty?" (McDonald 1985, 229). Most, however, anticipated a nation of freeholders and their families, and the political role of laborers remained a nonissue, the status of nonslave labor under the Constitution an invisible clause.

These arrangements and assumptions were the institutional ground upon which future labor conflicts would be staged. Within a half-century, the great compromise among the states would face its severest test in the prolonged conflict over slave labor. For several decades following that, the Constitution's original conception of public employment would contend with competing plans for a modern bureaucracy. During the New Deal, constitutional resistance to labor legislation by judges, based on common law rights and on the powers of states, would result in the constitutional diminishment of the powers of states, of common law rights, and of judges.

This said, it points to the insufficiency of the explanation proffered. The Constitution made arrangements and assumptions affecting social subjects besides labor—for instance, religion and commerce. Why did changes in labor relations in particular have the impact claimed? To answer this, a (mainly) *historical* explanation becomes necessary: the importance of labor in American constitutional development is explained by the point in English constitutional development at which the United States government came into being. American states and territories formally received into their own legal systems the common law and statutes of England as of some stated time (e.g., the immigration of a colony's first settlers, national independence, the adoption of the state constitution). English common law in place at the time of the U.S. Constitution's adoption was, by the early nineteenth century, an accepted component of federal jurisprudence as well.

Crucial for subsequent American constitutional development was the occurrence of prior English development in stages, corresponding to the major upheavals in English history. Beginning with Henry VIII's move against Rome, for which assent was obtained in the form of parliamentary statutes, and gathering steam during the late seventeenth century when the rising commercial classes consolidated their position with statutes on monopolies, banking, and land development, court-regulated hierarchies of personal relations that once ordered the whole of feudal society according to precepts of common law were successively dismantled, and final authority for their affairs relocated in Parliament (Orren 1991).

At the times set for designating which English law would be received into American law, however, this dismantling was far from complete. Religious and commercial affairs and the regulation of officeholders were by then the jurisdiction of Parliament; pari passu, these subjects were governed by statutes in the United States (in religion's case, in those states that did not constitutionally provide for strict separation). By the same token, personal relations still under the authority of the judiciary in England—Blackstone organizes them as master and servant, husband and wife, parent and child, guardian and ward—transferred into the United States as judicially enforceable rights and duties at common law. Although there were English statutes on those subjects at this time, they implemented or clarified provisions in common law, which law was incorporated into American jurisprudence largely intact.[18]

The inherited division of sovereignty—between legislatures and courts—was buttressed by English rules of statutory interpretation. Statutes in derogation of common law were to be construed narrowly; legislative remedies were to be judged valid or "absurd" according to the particular category of relations affected (Smith 1848, 676–77). Following Blackstone, American treatise writers designated the personal relations above as "natural," and judges treated statutes for their regulation as suspect; business corporations, on the other hand, were deemed "artificial," subject to democratic renovation (Kent 1832). Although in principle these divisions were independent of the division between national and state authority, all were tied, historically and structurally, to the same particularized ordering of society by common law.

Only a few American statutes governing relations between masters and (nonslave) servants were passed prior to the Civil War. Under common law rules, however, these (e.g., the 10-hour statutes) could not be enforced against evidence that the parties had come to some other agreement, which was always present. Common law rights of husbands over the work of their wives were afforded parallel protection against legislative interference (Zeigler n.d.). Slavery had been declared a creature of "positive" (statutory) law—indeed, "repugnant" to the common law—shortly before the American Revolution.[19] However, in both Northern and Southern courts, slavery statutes were enforced by common law actions (e.g., habeas corpus and recaption, an ancient writ to recover villeins) as a matter of right.

The reception of English law remained the touchstone: at the height of "liberty of contract" in the late nineteenth century, usury statutes were permissible because their existence in England antedated the Constitution. In the case of public employment, where legislatures possessed expansive authority to establish rules for government service, common law rights of officeholders that transferred into the new United States remained an important supplement to statutory provision. When officeholding statutes by one method or another were voided or where statutes were silent, their ancient vitality continued and could be asserted in court to block contrary directives and denial of privilege.

Against this background, it will be seen that changes in labor relations implicated broader constitutional matters than the regulation of the nation's work force. Rather, they raised questions of how far democratic decision making would extend across the terrain of American society. The replacement of particularized governance by the universal political institutions anticipated in liberal ideology is a fair summary of American constitutional history. The process tracks the dislodgement of work relations shored up by the Court in *Marbury*, in *Dred Scott*, in *Lochner*. The removal of aristocratic styles of public employment was the premise of the mass-based political party; the removal of slavery entailed the "national majority" and the Fourteenth Amendment; the removal of the hierarchy of master and servant removed common law barriers to legislation. The breakdown of divisions was evident in the mid–twentieth century: when the Civil Rights movement addressed lingering vestiges of slavery, it propelled a generalized "rights revolution."

The two answers provided thus far explain why labor relations was a subject of constitutional dimensions and why changes in labor relations reached beyond the affairs of the workplace. There is, however, a third answer, one able to encompass both of these, and also to account for why labor issues may prove constitutionally salient into the future. This answer is (mainly) *theoretical*: it proposes an abstract starting point for a general analysis of state-society relations in terms of the *primacy of labor* (Orren 1991, 19).

The primacy of labor builds on the idea that "labor" and "work" may be understood comprehensively, as they were before the eighteenth century, to include the varied activities during any historical period that are performed under the order or direction of someone else (Williams 1983, 177). From there proceeds the following observation: labor is a bridge between the realm of government institutions and elites and the ongoing activities of social life. Governments in whatever form depend for their functioning on the labor of individuals, both in their internal operations and with respect to diverse goals they may pursue. Individuals perform work according to established routines, within organizations, and for rewards, sanctioned by the state. A change in these activities or a change in demands on the part of government elites will cause adjustments on one or both sides.

An idea related to the primacy of labor has been presented by Talcott Parsons as part of his system of social action. According to Parsons, the element of mutual reliance on labor activities by state and society is expressed in the concept of *services*. Within this system, the personal commitment to work is described as the primary output of the household to the polity, the polity being that social subsystem dedicated to goal attainment; and the combination of this personal commitment with other factors is the primary output of the economy to the polity (Parsons 1969). Parsons' notion is more abstract than what is proposed here, which contemplates specific institutional settings—the government, the workplace, the family—although my perspective would not be inconsistent with his framework but rather a concrete instance of it.

Where Parsons sees services obtained in society through the processes of exchange, the primacy of labor is proposed to distinguish modes of labor regulation within given regimes, allowing for coercion as such, not as a species of exchange. The unwillingness to characterize labor relations in terms of exchange is related to the definition of its essential condition, subordination. Elements of exchange may often—and in some labor regimes may regularly—be present; but the employee performs within a structure of authority and may be dismissed for nonperformance or other reasons, often through the will of one side. By contrast, property relations are distinguished by the condition of autonomy. Property is, likewise, not autonomous in an absolute sense, being subject to both legal and market forces; but autonomy (or "relative autonomy") characterizes property, and that allows exchange, rather than coercion, to be its dominant transaction with the outside world. Labor and property are related: property is often a precondition of hiring labor; labor may be a means to property; employees may be treated in law as if they were property; employment may exhibit a security akin to property.

Changing social arrangements under the American Constitution may be analyzed comfortably within this framework. The systems of free and slave labor, for example, coexisted on the presumption of propertylike characteristics in the system of public officeholding, imposing personal liability for the performance of duty on those who enforced controversial laws. The widening project of national government would modify all three. The same is true of the background of English development. The clerical institutions challenged by Henry VIII performed a variety of services still associated with the public domain (e.g., teaching, administering wills, poor relief). The monopoly privileges opposed by England's "new men of commerce" in the mid–seventeenth century had been commonly assigned to royal clerks and other functionaries in lieu of salaries and pensions (Price 1913).

The third answer, the *primacy of labor*, will be problematic only if its analytic generality tends to water down the impact of the specific argument, namely, that labor relations, as colloquially understood in contemporary American culture, have driven major constitutional change. There is no contradiction between the general and the specific. The changing structures and activities of work have been no less potent elements in political change for their terminological narrowing over centuries.

RESPECTING PROPERTY

Granted the connection claimed between labor relations and constitutional turning points—and even if one accepts the explanations tendered—there remains a conspicuous hole in the account. Where are *Fletcher* v. *Peck* (1810), *Dartmouth College* v. *Woodward* (1819), *Sturges* v. *Crowninshield* (1819), *McCulloch* v. *Maryland* (1819), *Gibbons* v. *Ogden* (1824), and *Charles River Bridge* (1837)? How can these celebrated cases—and later cases like them—be excluded from the discussion when they are the best evidence for the alternative social perspective, which is that American constitutional development has concerned, above all, property in its various forms and its protective adjustment to a democratic order?

No exception is taken here to the observation that property has "enjoyed an elevated position in the pantheon of constitutional values recognized by the highest Court in the land" (McCann 1982, 147). However, I shall put forward and briefly defend two propositions. First, the Court's famous decisions arising from disputes over property have served to affirm and clarify the constitutional status quo; they are not turning points in the sense of the decisions enumerated earlier arising from disputes over labor. As with labor, the association between property and the consolidation of existing constitutional arrangements was not fortuitous but historically structured. Second, these property cases themselves reveal the imprint of an earlier period, when activities that in the nineteenth century were thought of as property had been seen in relation to the state as services—that is, in terms compatible with an argument for the primacy of labor.

Evidence for the proposition that the Court's property decisions clarified and reinforced the status quo will be found in *Fletcher* v. *Peck,* which overturned a Georgia statute repealing an earlier sale of lands as a violation of the contracts clause of the U.S. Constitution. Georgia's ceding the disputed lands to Congress, the raging rhetoric of party factions, the corruption surrounding the original grant—all account for the role of *Fletcher* v. *Peck* in the dramaturgy of states against nation, Congress against presidency, virtue against power. Because the Court had struck down state legislation only once before, its decision was a stinging reminder of the power of the national judiciary.[20] Decided seven years after *Marbury,* however, there was no longer a question that the Court had authority to invalidate unconstitutional statutes. On this point, the *Fletcher* opinion was unanimous, gathering even the vote of Justice William Johnson of South Carolina, Jefferson's appointee, who was often at odds with the Marshall Court's consolidationist tendencies.

Perhaps the more important issue for the status of property in constitutional development is whether the decision in *Fletcher* v. *Peck* constrained future state legislation. The Court held that the contracts clause covered benefits that statutes bestowed, and it legally equated "grants" with "contracts" and "public contracts" with "private contracts." But this was not a novel ruling in the United States, having been foreshadowed by an earlier Marshall opinion.[21] Nor was there serious dispute that a legislature could not, apart from unusual circumstances like the corruption attending the Yazoo grant, take land from its owners without compensation. Even the most bitter opponent of the outcome in *Fletcher,* John Randolph, would have agreed with Marshall that such action would be contrary to "general principles which are common to our free institutions"—in Justice Johnson's words, "the reason and nature of things" (1810, 139, 143).[22]

Statements like these were familiar in the Court's jurisprudence. Justice Chase, for example, in *Calder* v. *Bull* said there "are certain principles in our free republican governments which will determine and overrule an apparent and flagrant abuse of legislative power; as to . . . take away security for personal liberty, or private property, for the protection whereof the government was established" (1798, 388). Events like Shay's rebellion and several state laws under the Confederation had indicated a more radical tack; they were exactly what the Constitution was designed to prevent and had prevented successfully. No egregious case like *Fletcher* had arisen in two decades; the founding generation had by now stopped fretting about the excesses of republicanism.[23]

Additional evidence for the consolidating, rather than restructuring, role of the Court's property decisions is provided by *Dartmouth College* v. *Woodward.* Overturning a New Hampshire statute that changed provisions in a prerevolutionary royal charter, *Dartmouth College* held that the Constitution's contract clause protected not only individual property owners but also corporations with charters granted for unlimited periods. The decision provided scaffolding for future business organization and, as such, was a vital holding in favor of economic development. But it marked no constitutional turning point, either in doctrine or in principle. No new substantive rights were involved; corporations as well as individuals remained subject to eminent domain and other traditional state encroachments.

Justice Story, in his concurring opinion, invited legislatures to write their powers of revocation into corporate charters they might grant in the future, a suggestion quickly followed by most states. But the precaution had only symbolic significance. Corporate property was equally secure before and after *Dartmouth College.* In this respect, what the decision affirmed was not so much the Constitution's harmony with property values but rather its harmony with a culture that could be expected to protect property without the judiciary's need to intervene.

A third decision, *Charles River Bridge* v. *Warren Bridge,* established that United States courts would construe existing charters strictly in all cases that determined property rights against the state. But again, especially given the proactive measures available to legislators after *Dartmouth College,* it is difficult to regard *Charles River Bridge* as a constitutional departure. *Dartmouth College* had concerned a charitable and educational enterprise that owned land and was associated with religion; the New Hampshire legislature had altered the physical site, the governing board, and the very name of the college. *Charles River Bridge* concerned rights to a public waterway, a subject anciently under state aegis; the plaintiff's bridge was slated to become state-owned after a period of years. The Court had simply declined to find, against a new and competing bridge, unstated monopoly rights that the plaintiff company claimed under common law and under a disputed seventeenth-century grant.

English precedents with respect to tolls, rates, and dock dues held that where the public was charged with a burden, the legislature's intention to impose it must be explicitly and distinctly shown (Dwarris

1848, 646–48). Several years before, Justice Marshall had endorsed this rule for the Court as it applied to taxation.[24] *Charles River Bridge* extended it to the state's interest in public improvements. The decision demonstrated the Taney Court's aggressive commercial spirit against old monopoly privileges and favored some property holders over others. Its principal impact, however, like that of *Dartmouth College*, was on corporate lawyers and other legislative draftsmen. Like *Fletcher* v. *Peck* and *Dartmouth College*, *Charles River Bridge* was an occasion to fit changing economic reality within existing constitutional structures; it represented no about-face in national direction, no leaning against any constitutional or social wind.

These conclusions could be extended to *McCulloch* v. *Maryland*. McCulloch gave full expression to the doctrine of implied congressional powers that Alexander Hamilton asserted during the chartering of the first Bank of the United States and Justice Marshall supported on the Court as early as 1805.[25] The decision also supported the growing power of the central judiciary presaged in *Marbury* v. *Madison*, as did *Gibbons* v. *Ogden*, which declared Congress's precedence over the states in the regulation of franchises on interstate waterways. This was a signal holding but a holding well in keeping with the original constitutional aim of avoiding state hindrances to commerce.[26] *Sturges* v. *Crowninshield's* overturning a state bankruptcy statute was a temporary setback to commercial interests but only by virtue of a Court majority against a policy imposed retroactively; its holding was for all practical purposes reversed within a decade.[27] None of these decisions strayed from the English constitutional development that Americans considered their patrimony. This included a latitude for legislators to regulate commerce free of common law precedent, a reticence toward (though not a barrier against) monopoly, and a hearty respect for property rights as a social principle. Controversies over particular property claims led to lawsuits; no antiproperty precepts existed or emerged; no reconstitution was entailed or envisioned.

Earlier, in order to explain their constitutional effects, I have placed the Court's labor decisions in historical and theoretical context. A brief imposition of the same framework on the property decisions will suggest its analytic range. Carrying forward, rather than changing, fundamental law, the property decisions bear the earmarks of previous transitions, before the framing of the Constitution.

Speaking only of private commerce, and setting aside public facilities like bridges, grist mills, and toll roads of ancient usage: medieval financial and industrial enterprises, like all other social activities, were at the disposal of the monarch for a diversity of political and economic ends. Prior to their reigning-in by Parliament, Elizabeth and the early Stuarts promoted monopoly privileges as an important method for compensating supporters of the royal house. Increasingly, these projects took the form of joint-stock companies, making them the legal forerunners to early corporations in the United States. The overall pattern—of accomplishing government purposes through economically lucrative patronage—was much older and pervaded English public administration into the nineteenth century.

Invoking this history, Justice Story's dissent in *Charles River Bridge* turned on whether the first bridge company's grant was in the nature of a "bounty," for which no services were required on the part of the recipient, in which case, based on English precedents, the state must be conceded to hold in reserve all unspecified rights; or whether the grant was for services, in which case contracts must be construed in favor of the grantee. On this reasoning, he argued that the company was entitled to its tolls as ongoing compensation for having built the bridge (1837, 589). Story agreed with Taney that the practices of English royalty could have no binding effect on American corporations, and his argument about services was turned aside by the majority. Nevertheless, it is significant that a judge as distinguished as he continued to sort out rights and duties on the old design.

Because *Charles River Bridge* concerned a public waterway, it is perhaps more convincing to see the same idea of services in *Dartmouth College*, which arose from a private undertaking, though on a charter from George III. For its defense, the New Hampshire legislature relied on the distinction between protections afforded landed property—at common law and in such American precedents as *Fletcher* v. *Peck*, *New Jersey* v. *Wilson* (1812), and *Terrett* v. *Taylor* (1815)—and the rights of corporations.[28] Against Webster's argument that the College trustees, who were the plaintiffs in the case, held their powers as their own inviolable property, counsel for New Hampshire asserted that corporate trustees were "public agents." Just as the state could, upon harsh penalties, constitutionally force citizens to serve in the militia and in town offices, it could likewise "compel [the trustees] to serve;" as agents, trustees had no more right to complain if their number were increased than did judges if the judiciary was enlarged. The Superior Court of New Hampshire voiced the same labor-based idea: "These trustees are the servants of the publick, and the servant is not to resist the will of his master, in a matter that concerns that master alone."[29]

Chief Justice Marshall pursued a different line of reasoning in his *Dartmouth* opinion to find rights for the trustees. Shifting attention to the original donors of the property, he found that a contract existed between them and the king, based on an expectation of immutable powers in the trustees as the donors' representatives. He went on, however, to deny that the trustees lacked beneficial interests of their own. According to the terms of the charter, said Marshall, some trustees would be professors with salaries, in which case they could claim violation of their own contracts for services (1819, 339). In other words, rights against the state would be vested in employees, not of the state directly, but of the corporation that the state charter had brought into being. Marshall refrained from pressing this argument, saying it was sufficient to rest his decision on the trustees as the representatives of the donors. Had he persisted, he might have counted among his accolades the title of founding father of American labor law.

The point in all this is not to suggest that legislators in the early nineteenth century planned forcibly to draft the services of businessmen or that judges could not tell corporations apart from workshops. What is important is that they were closer to a period when most activities were characterized by subordination rather than autonomy, compensation as well as profits, insecurity instead of protected rights. Several centuries earlier, land had become largely exempt from these disabilities. Beginning in the mid-seventeenth century, commercial entrepreneurs obtained similar privileges for their holdings. However, for some time afterward, it did not seem awkward to speak of business and corporations in the old terms, as services rather than property, in contrast to today when their interests seem so firmly entrenched.

In the study of institutions, history and theory are mutually illuminating. Against the English background, property relations under the U.S. Constitution are seen to have been established and paradigmatic, labor relations to have been unsettling and reconstructive. The prominence of labor relations, however—and its explanation—encourages a scrutiny of the Court's property decisions that uncovers an understanding older than the Constitution, according to which activities later defined as property and culturally synonymous with individual autonomy were, in an earlier period, conditioned on a subordination associated more recently with employment. On the one hand, this historical uncovering is itself a process of interpretation, guided by abstract, i.e., transhistorical, definitions of labor and property. On the other hand, the primacy of labor in this uncovering takes on added temporal, i.e., historical, dimension.

History and theory merge in the assertion that employment, with its characteristics of subordination, compensation, and discharge, is a model on which societies have historically organized purposeful activities of all kinds and at all social levels. This suggests that labor relations are indefinitely various and that their description will offer a full and comparable picture of political institutions, in time and over time.

Notes

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1. Recent among a myriad of titles that embrace this theme in diverse ways are Nedelsky 1990 and Ackerman 1991.

2. These core principles include individual self-determination, no established ranks in society, and the political sovereignty of the citizenry.

3. Important groundwork for this perspective is provided by the rich scholarship of the last two decades on labor and the American state (see, e.g., Forbath 1989; Hattam 1993; Klare 1978; Tomlins 1985). In a sense, the most innovative thrust of these studies cuts in the opposite direction from the discussion here; that is, it highlights the influence of state structures and policies on the strategy and fortunes of the labor movement, rather than the reverse. On the other hand, by actively exploring state-society linkages which labor's failure to challenge had preoccupied earlier scholars), this recent work opens to theorizing the relationship of government and labor historically and with regard to a fuller range of work relations. In a related vein of research, Tushnet (1981) and VanderVelde (1989) have uncovered new political-legal connections between slavery and white labor. Though not within a self-conscious labor framework, Cover (1975) offers a fascinating juxtaposition of slavery and public employment (judges).

4. Such a listing of turning points by no means exhausts the possibilities of the labor perspective. It will hardly be necessary to dwell on the Civil War amendments—their pervasive constitutional impact and their direct association with black slavery—in order to call up their relevance to our argument. In that connection, one could list certain "antilandmarks"—decisions that signal the arresting of constitutional changes underway, such as *United States* v. *Cruikshank* (1876) and the *Civil Rights Cases* (1883). These cases are interesting not only for their treatment of slavery's residues but for their reliance on the employment relation between government and its officers to define state action and curb national protection of the newly freed. It is also possible to establish labor's presence in Court decisions that are not so much markers of constitutional realignment as they are foundational to later development. In this category, for instance, is the place of labor radicalism in the emergence of First Amendment jurisprudence at the end of World War I, exemplified by *Schenck* v. *United States* (1919) and *Abrams* v. *United States* (1919).

5. A study of the U.S. Supreme Court's invalidation of state statutes between 1879 and 1910 indicates that the antibusiness measures disputed were, on the whole, upheld by their local state high courts (Gates 1987, 271). By contrast, a listing of state labor statutes passed between 1885 and 1900 shows a pattern of division between legislatures and judiciaries: approximately three times as many were invalidated by state high courts as were upheld (Forbath 1989, app. A). The U. S. Supreme Court sustained almost as many instances of government price fixing as it invalidated, a considerably more positive record than the Court's treatment of labor statutes (Strong 1986, 273–92). The exceptions to labor's dismal record before the Court are two decisions upholding hour limits for special categories: *Holden* v. *Hardy* (1898), about underground miners in Utah and *Muller* v. *Oregon* (1908), about Oregon women.

6. *Jones* v. *People* 1884.

7. *Godcharles* v. *Wigeman* 1886.

8. Justice Field might have had a better argument if the law had made the monopoly the only employer of butchers. But it provided for them to rent space from the franchisee.

9. An interesting exception suggesting the prominence of labor in the Court's decision making is found in the regulation of grain futures. Congress initially regulated grain-futures sales by legislating prohibitive penalties against practices other than those prescribed. This measure aimed to circumvent the Court's earlier interdiction—in *Hammer* v. *Dagenhart* (1918), which struck down a national child labor law—against using the commerce power to perform police functions. The same "taxation" approach was used in a rewritten child labor law, after which both statutes were struck down by the Court on the same day, in *Hill* v. *Wallace* (1920) and *Bailey* v. *Drexel Furniture* (1920). Congress then rewrote the grain-futures act as a measure designed to prevent obstructions of commerce, and it was upheld in *Stafford* v. *Wallace* (1922).

10. The phrase is from *In re Neagle,* which upheld the legality of employing a U.S. marshal to guard a Supreme Court justice (1890, 69). The other instance was *In re Debs,* upholding executive authority "to enforce in any part of the land the full and free exercise of all national powers and the security of all rights entrusted by the Constitution to its care" (1895, 181–82). Plenary authority was affirmed for Congress in similarly sweeping terms in the *Chinese Exclusion Case,* upholding a statute that contravened a U.S. treaty with China by prohibiting the importation of Chinese laborers (1889, 603–4).

11. Examples are *Wickard* v. *Filburn* 1942 and and *Heart of Atlanta Motel* v. *United States* 1964.

12. On this requirement as applied to the Clayton Act, sec. 20, see *Duplex Printing Press* v. *Deering* 1921.

13. *Hammer* v. *Dagenhart* 1918.

14. *McLaurin* v. *Oklahoma State Regents* 1950; *Sweatt* v. *Painter* 1950. The plaintiff's brief in *Sweatt* v. *Painter* states, "If [Negroes] have the chance to rise above the cotton picking, the manual labor, and the domestic service to which our white society has consigned their race, it is almost as important to them as life itself that they have the opportunity to do so" (quoted in Kluger 1977, 276). "Educational Inequalities Must Go!," an early position paper written in 1935 by NAACP leader Charles Houston, declared, "One of the great tragedies of the Depression has been humiliation and suffering which public authorities have inflicted upon trained Negroes, denying them employment at their trades on public works and forcing them to accept menial low-pay jobs as an alternative to starvation" (ibid., 193).

15. *Patterson* v. *McLean Credit Union* 1989 (limiting protection of Title VII of the 1964 Civil Rights Act to employment discrimination at the point of hiring); *Ward's Cove Packing* v. *Antonio* 1989 (shifting the burden to the employee to prove an inadmissible reason for stratifying workers de facto along racial lines); and *City of Richmond* v. *J. A. Croson Company* 1989 (imposing an equal-protection bar to affirmative action based on race in the absence of direct evidence of past racial discrimination). The last case denied that allocating a proportion of city construction contracts to minority businessmen bore the necessary relation to difficulties minorities had participating in the industry due to hiring practices and to blocked access to training programs and labor unions (p. 498–99).

16. *Transportation Union* v. *Long Island Railroad* 1982.

17. The litigation over reproductive rights, beginning with *Griswold,* has been fueled by a concern on the part of both litigants and judges for doctors' freedom to perform their expert labor without fear of penalty (Garrow 1994). A continuation of this line of decisions may be expected in Court cases determining the rights of persons with nontraditional sexual orientations, and in particular when engaged in the work of military service.

18. American law did not receive all English statutes as of a given date but only those that were "general" in nature and "applicable" to the condition of the new nation. Because these qualifiers were satisfied piecemeal, as judges applied them in litigation, it is possible only to surmise why certain legislation was discarded. Presumably the English master-and-servant statutes fell by the wayside on both counts, being detailed as to industry and locale.

19. *Somerset* v. *Stewart* 1772.

20. The first instance was a state law that conflicted with a federal treaty, *Ware* v. *Hylton* (1796).

21. "This [sale of land to settlers, providing conditions of residence, etc.] is a contract; and though a state is a party it ought to be construed according to those well established principles which regulate contracts generally" (*Huidekoper's Lessee* v. *Douglass* 1805, 70).

22. *Fletcher's* holding was likewise consistent with contemporary English equity opinion on the security of bona fide purchasers of land. *Stanhope* v. *Earl Verney* 1761, 85; *Jerrard* v. *Saunders* 1794, 458; see also Pomeroy 1941, 3:6–7. Parliament's authority even to rescind commercial grants had been recently challenged, in constitutional terms that paralleled the American debate. See the debate on Fox's East India Company bill, especially the remarks of Edmund Burke, the bill's real author (*Parliamentary History* 1814, 23:1131 ff.). The distinction may have played a part in the minds of Randolph and other Yazoo repealers, for the greater part of the property was held not by settlers but by New England–based financiers and speculators (McGrath 1966, 38). In any case, *Fletcher* did concern land, and it set no new precedent in this respect.

23. Compare Justice Chase's upholding a Georgia statute against charges that it was a bill of attainder. Although the statute had resulted in the confiscation of property, Chase distinguished it as follows: "There is . . . a material difference between laws passed by the individual states, during the Revolution, and laws passed subsequent to the organization of the federal constitution. Few of the revolutionary acts would stand the rigorous test now applied" (*Cooper* v. *Telfair* 1800, 19). For the agreement among American leaders on the sanctity of property values see Nelson 1987.

24. *Providence Bank* v. *Billings* 1830.

25. *United States* v. *Fisher* 1805.

26. The Court's decisions on interstate commerce were regularly tangled up with questions of slavery. In the background of *Gibbons,* for example, was the problem of Southern states arresting free black seamen who came ashore in Southern cities. See *Elkison* v. *Deliesseline* (1823). This case is referenced in *Gibbons* and in Justice Johnson's dissent (1884, 215–16, 230–31). Another link to labor in the commerce cases concerns immigration. Thus, *New York* v. *Miln* (1837) upheld the right of a state to regulate passengers on seafaring vessels, based among other things on the prospect of unemployed immigrants becoming a charge on the city.

27. The reversal was in *Ogden* v. *Saunders* (1827).

28. Farrar 1819, 82–98. *New Jersey* v. *Wilson* held that an exemption from taxation granted Native Americans as part of a treaty could not be rescinded when title passed to new owners. *Terrett* v. *Taylor* held that the Virginia legislature could not void an earlier statutory grant of land to the Episcopal Church. The changing character of "things" protected by core property forms is the theme of a different historical treatment by a political scientist of the Court's property decisions (Brigham 1983).

29. Farrar 1819, 36, 201–2, 217. A similar argument was made by William Wirt, in answer to an appeal to the U.S. Supreme Court. The trustees, he said, had given "their time and labour" to the college, that was all: "Every society has a right to the services of its members in places of publick trust and duty" (ibid., 292).

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THE NEW DARWINIAN NATURALISM IN POLITICAL THEORY

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There has been a resurgence of Darwinian naturalism in political theory, as manifested in the recent work of political scientists such as Roger Masters, Robert McShea, and James Q. Wilson. They belong to an intellectual tradition that includes not only Charles Darwin but also Aristotle and David Hume. Although most political scientists believe Darwinian social theory has been refuted, their objections rest on three false dichotomies: facts versus values, nature versus freedom, and nature versus nurture. Rejecting these dichotomies would allow the social sciences to be linked to the natural sciences through Darwinian biology.

In recent decades, there has been a revival of Darwinian social theory among many social scientists (Barkow, Cosmides, and Tooby 1992; Degler 1991; Fox 1989; Frank 1988; Masters and Gruter 1992; Maxwell 1991; Maryanski and Turner 1992; Schubert and Masters 1991; Smith and Winterhalder 1992; Wright 1994). The early influence of this intellectual movement in political science became evident in the work of a few political theorists such as Fred Willhoite (1976), Robert McShea (1978), and Peter Corning (1983). More recently, this new Darwinian political theory has been elaborated by McShea (1990), Roger Masters, and James Q. Wilson.

The political naturalism of these theorists is not only Darwinian but also Aristotelian and Humean: they agree with Aristotle that human beings are by nature social and political animals; they agree with David Hume that human beings are by nature endowed with a moral sense; and they agree with Charles Darwin that human sociality and morality are rooted in human biology.

Most political scientists, however, assume that such a political naturalism grounded in Darwinian biology is refuted by three objections. First, Darwinian naturalism ignores the radical separation between *is* and *ought* by deducing moral values from biological facts. Second, it promotes a biological determinism that denies the human freedom presupposed by morality and law. Third, it fails to recognize that human morality and politics are products of social learning rather than biological instincts. I shall defend the new Darwinian naturalism by arguing that these objections rest on three false dichotomies: facts versus values, nature versus freedom, and nature versus nurture.[1]

NATURAL MORALITY

One of the most pervasive assumptions in the social sciences is that there is an unbridgeable gap between *is* and *ought*. This is often called "Hume's law" to indicate that Hume was the first to discover it. Because of this separation between judgments of fact and judgments of value, it is thought, scientific objectivity in the social sciences dictates moral relativism (Brecht 1959). Consequently, one of the most common objections to any Darwinian theory of human morality as rooted in human nature is that this fallaciously infers moral values from natural facts (Kitcher 1985). Even many of those who propose Darwinian theories of ethics accept the fact/value dichotomy: "The very last thing the Darwinian wants to do is break Hume's law by denying that there is a genuine 'is/ought' distinction" (Ruse 1986, 251).

I would argue, however, that far from separating facts and values, Hume showed how moral judgments could be grounded in certain facts of human nature. This explains why Darwin and the new Darwinian political theorists can incorporate Hume's theory of the moral sense into their evolutionary account of human morality.

The common interpretation of Hume as having separated *is* and *ought* depends on only one paragraph in his *Treatise of Human Nature* (1888, 469–70). Some Hume scholars have shown that if one considers carefully both the textual and historical contexts of this paragraph, one sees that the common interpretation is wrong (Buckle 1991, 282–84; Capaldi 1966, 1989; Martin 1991). The textual context makes clear that Hume's claim is that moral distinctions are derived not from pure reason alone but from a moral sense. The historical context makes clear that Hume is restating Francis Hutcheson's criticisms of some early modern rationalists such as Samuel Clarke and William Wollaston, who believed that moral distinctions could be derived from abstract reasoning about structures in the universe that were completely independent of human nature.

Far from denying that moral judgments are judgments of fact, Hume claims that moral judgments are accurate when they correctly report what our moral sentiments would be in a given set of circumstances. Moral judgments do not have *cosmic objectivity* in the sense of conforming to structures that exist totally independently of human beings. Yet neither do moral judgments have only *emotive subjectivity* in the sense of expressing purely personal feelings. Rather, as Nicholas Capaldi (1989) has argued, moral judgments for Hume have *intersubjective objectivity* in that they are factual judgments about the species-typical pattern of moral sentiments in specified circumstances.

Hume compares moral judgments to judgments of secondary qualities such as colors (1888, 469; idem 1985, 233–34). My judgment that this tomato is red is true if the object is so constituted as to induce the impression of red in normally sighted human beings viewing it under standard conditions. Similarly, my judgment that this person is morally praiseworthy is true if the person's conduct is such as to induce the sentiment of approbation in normal human beings under standard conditions. Just as an object can *appear* red to me when in fact it is not, so a person can *appear* praiseworthy to me when in fact he is not. The moral judgment whether some conduct would give to a normal spectator under standard conditions a moral sentiment of approbation is, Hume insists, "a plain matter of fact" (1902, 289). The moral sentiment itself, however, is a feeling or passion rooted in human nature that cannot be produced by reason alone.

When Hume declares that "reason is, and ought only to be the slave of the passions" (1888, 415), he is not promoting emotivist irrationalism. As the context of this remark makes clear, he believes that reason can *direct* action but not *motivate* it: "the impulse arises not from reason, but is only directed by it" (p. 414). When our passions are accompanied by false judgments, reason can properly correct them: "The moment we perceive the falsehood of any supposition, or the insufficiency of any means our passions yield to our reason" (p. 416). "Reason and judgment may, indeed, be the mediate cause of an action, by prompting, or by directing a passion" (p. 462); consequently, "reason and sentiment concur in almost all moral determinations and conclusions" (1902, 172). For example, reason might instruct us as to how justice could be useful to society, but this alone would not produce any moral approbation for justice unless we felt a sentiment of concern for the happiness of society (pp. 285–87).[2]

Aristotle agrees with Hume about the primacy of desire or passion in motivating human action. "Thought by itself moves nothing," Aristotle believes, although reason can guide the desires that do move us. Desire always moves us, but thought never moves us without desire (*De Anima* 433a10–31).[3] Deliberate choice (*proairesis*), therefore, requires a conjunction of desire and reason into "desiring thought" or "thinking desire" (*Ethics* 1139a36–b6).[4] Like Hume, Aristotle also believes that the passions manifest a natural moral sense, which is particularly evident in moral passions such as anger and indignation, as I have shown elsewhere (Arnhart 1981, 102–5, 114–34). Aristotle identifies the moral passions as praiseworthy states of character. Although they are not "virtues in the strict sense," because they do not arise by deliberate choice, they are the natural dispositions to morality that become moral virtues through the cultivation of proper habituation and prudential judgment (*Eudemian Ethics* 1233b16–34b11; *Ethics* 1144b1–18). Cicero restates Aristotle's position in explaining that all animals act according to nature in satisfying their natural desires and that human beings act according to their distinctively human nature in satisfying their "primary natural desires" through the moral and intellectual virtues (Cicero, *De Finibus* 2.33–41 and 107–10; 3.16–22; 5.17–20 and 24–72). Thus, Aristotle recognizes—but does not elaborate—the psychological basis of ethics in the moral passions that is elaborated by David Hume and Adam Smith (Berns 1994).

Hume's moral sense is rooted in the natural social affections of human beings. In rejecting the "selfish system of morals" of Hobbes and Locke, who deny the natural sociality of human beings and argue that moral inclinations are utterly artificial products of a social contract, Hume insists that although the progress of the moral sentiments does require "the artifice of politicians," this political artifice succeeds only with the support of nature. If human beings were utterly selfish and solitary in their nature, they could never develop the social sentiments necessary for moral life: "The utmost politicians can perform, is, to extend the natural sentiments beyond their original bounds; but still nature must furnish the materials, and give us some notion of moral distinctions" (Hume 1888, 295–96, 500, 619–20; idem 1902, 214–15, 296–97).

Aristotle believes that the natural sociality of human beings and other political animals is an extension of parent-child bonding.[5] The various forms of friendly feeling that unite human beings as individuals, as fellow citizens, and as members of the human species radiate out from the natural affection between parents and offspring that human beings share with other animals whose offspring require intensive and prolonged parental care (*Generation of Animals* 753a8–14; *History of Animals* 612b18–20b9; *Ethics* 1155a1–33, 1159a27–37, 1160b23–62a29; *Rhetoric* 1371b13–26). Consequently, "in the household are first found the origins and springs of friendship, of polity, and of justice" (*Eudemian Ethics* 1242b1–2). Insofar as justice coincides with friendship, the claims of justice vary in proportion to the nearness of attachments (*Ethics* 1155a16–29, 1159b25–60a8, 1165a14–36). One's obligations are stronger to closer relatives than to more distant ones and stronger to close friends and fellow citizens than to strangers, although some friendly feeling is possible towards all members of one's species. There can be a kind of sympathy among animals of the same species; and this is especially true for human beings, so that "we praise those who love their fellow human beings" (*Ethics* 1155a20–21). Still, the humanitarianism of human beings will always be difficult to cultivate and almost always weaker than their egoism, their nepotism, and their patriotism.

Similarly, Hume regards the dependence of offspring on parental care as the natural root of human sociality and morality (1888, 483–86, 570–73; idem 1902, 189–90, 192, 201–8, 240, 295–311). The natural moral sentiments that bind people in families can expand to embrace larger groups. Then, gradually, through the experience of mutual intercourse between societies, the boundaries of justice can enlarge as people discover the utility of extended social interdependence. This "natural progress of human sentiments" depends upon a natural sympathy or "sentiment of humanity"—a concern for the welfare of one's fellow human beings that extends in principle to all members of the human species. Of course, the concern for strangers will almost always be weaker than the concern for oneself and one's immediate family and friends, but the affection of humanity is strong enough to constitute the universal principle of morality. It is the "internal sense or feeling, which nature has made universal in the whole species" (Hume 1902, 169–73, 192, 218–29, 268–78).

If we accept the common view of Hume as having argued that we cannot infer what *ought* to be from what *is* the case, then it would seem that he contradicts himself by deriving morality from the natural inclinations of human beings. The contradiction disappears, however, once we see that the dichotomy between *is* and *ought* falsely attributed to Hume was actually first formulated by Immanuel Kant, who used it as an argument against the kind of ethical naturalism developed by Hume! Furthermore, once this point is understood, it becomes clear that while the proponents of Darwinian naturalism are Humeans, their critics are Kantians.

According to Kant, judging what *is* the case belongs to the "phenomenal" realm of nature, but judging what *ought* to be belongs to the "noumenal" realm of freedom (1956, 4–5, 18, 30–31, 99, 163–64; idem 1959, 4, 30, 44–45, 67–74, 80; idem 1965, 465, 472–79, 526; idem 1987, 286–87). (Kant employs the distinction in German between *sein* and *sollen*.) As opposed to Hume, Kant is a dualist. For Kant, the natural world is governed by causal laws that can be understood by natural science; and in this world there can be no free will, because every event must be determined by a causal mechanism. By contrast, in our moral experience, we praise and blame people in accordance with a moral law that transcends nature and is thus unknowable by natural science; and in this world we must assume free will, because moral judgment would be impossible unless we assumed that people were capable of freely choosing to obey or disobey the moral law. As moral agents, we obey categorical imperatives of what *ought* to be, but this *ought* expresses a moral necessity that has no place in nature: "When we have the course of nature alone in view, '*ought*' has no meaning whatsoever" (idem 1965, 473). As moral agents, human beings transcend the realm of nature and enter a realm of freedom that belongs to them as rational beings not governed by the laws of nature.

Kant's separation of *is* and *ought* treats morality as an autonomous realm of human experience governed by its own internal logic with no reference to anything in human nature such as natural desires or interests. He does this because he accepts the Hobbesian view of human nature. Since human beings are by nature asocial, selfish animals, they cannot live together in peace unless they conquer their natural inclinations by willing submission to moral rules devised by reason to pacify their conflicts (Kant 1970, 1987, 317–21; Simpson 1986).

When Darwin develops his evolutionary theory of morality, he adopts a Humean naturalism rather than a Kantian dualism. Darwin believes that his theory of evolution by natural selection will provide a biological explanation for what Hume, Adam Smith, and other Scottish moral philosophers identify as the moral sense (1936, 471–513, 911–19; idem 1987, 537, 558, 563–64, 619–29). When he begins his account of the moral sense in *The Descent of Man*, he quotes Kant's praise of moral duty, but he immediately suggests his departure from Kant by indicating that he will approach morality "exclusively from the side of natural history" (1936, 471).

Darwin sees that one of the central characteristics of the human species is the duration and intensity of child care. For that reason alone, human beings must be by nature social animals. The reproductive fitness of human beings requires strong attachments between infants and parents and within kin groups. Darwin believes this natural bonding of parents and children is the foundation of all social bonding and of the moral sense. "The feeling of pleasure from society is probably an extension of the parental or filial affections, since the social instinct seems to be developed by the young remaining for a long time with their parents; and this extension may be attributed in part to habit, but chiefly to natural selection" (1936, 478). For Darwin, this supports Hume's claim about the moral emotions of sympathy and benevolence as the basis for social cooperation (pp. 479–87).

Darwin also believes natural selection would favor mutuality and reciprocity as grounds for cooperation. Animals with the sociality and the intelligence of human beings recognize that social cooperation can be mutually beneficial for all participants. They can also recognize that being benevolent to others can benefit oneself in the long run if one's benevolence is likely to be reciprocated (1936, 443–44, 472, 479, 499).

Like Hume, Darwin explains human morality as emerging from the complex cooperation within groups competing with other groups, and thus only gradually and with great difficulty does human moral concern expand to include those outside one's own group. Throughout human history, justice has meant helping one's friends and harming one's enemies. Yet Darwin also agrees with Hume in claiming that as human beings are united into ever larger communities, their natural sympathy and benevolence can to some extent embrace all members of the human species (1936, 480–81, 491–95). One example of this extension of moral concern is the recognition that slavery violates the moral sense by treating some human beings as if they were not human (Chambers 1844, 379–83; Darwin 1936, 487–88; Hume 1902, 190–91; idem 1985, 383–91; Lincoln 1953, 2:222, 264–65, 271, 281–82; ibid. 3:541–42, 4:269).

Human beings are moral animals, Darwin explains, because they have the cognitive capacity to compare their desires or passions and judge that some are more important or enduring than others. As social animals, they feel concern for the good of others, and they feel regret when they allow their selfish passions to impede the satisfaction of their social passions. The word *ought*, Darwin concludes, signifies the consciousness that since some passions are more persistent than others, one cannot be fully happy if one does not satisfy those stronger passions (1936, 480–87).

The moral propensity to act for the good of others might seem to contradict natural selection. For example, courageous individuals naturally inclined to sacrifice their lives in defense of their community might often leave fewer offspring than cowardly individuals. Darwin believed, however, that in the competition between groups those with the more courageous members would often prevail (1936, 443–44, 497–98, 500). Not only courage but other moral dispositions to social cooperation might strengthen one group against others. Although many evolutionary biologists regard this appeal to group selection as one of Darwin's regrettable mistakes (Dawkins 1976; Williams 1966), some recent research suggests that Darwin was correct. Although natural selection can act within a group to favor selfish individuals over others, it can also act between groups to favor groups of altruists over others. Group selection occurs in those circumstances where selection between groups is stronger than selection within groups (Eibl-Eibesfeldt 1989, 90–103; Sober 1993, 88–117; Wilson 1983; Wilson and Sober 1989).

As soon as Darwin published his naturalistic theory of morality in *The Descent of Man*, he was attacked by St. George Mivart (1871). Mivart insisted on a Kantian separation between nature and morality. Although the human body could be explained as a natural product of biological evolution, Mivart contended, the human soul was a supernatural product of divine creation. And as an expression of the soul's transcendence of nature, human morality manifested a uniquely human freedom from natural causality.

Thomas Huxley (1871) immediately defended Darwin's ethical naturalism against Mivart's dualist critique, but Huxley moved later in his life, particularly in his famous lecture, *Evolution and Ethics* (Huxley 1894), toward a dualistic theory of ethics that Mivart (1893) recognized as his own. Huxley adopted the Hobbesian-Kantian view that since human beings in their natural state were selfish and asocial, the moral improvement of humanity required a self-abnegating denial of human nature (1894, 31, 44–45, 59, 68, 75–77, 81–85). Because of the "moral indifference of nature," one could never derive moral values from natural facts: "The thief and the murderer follow nature just as much as the philanthropist" (ibid. 59, 80). More recently, in the continuing debate over the ethical implications of Darwinian biology, biologists such as David Lack (1957), John Eccles (1989), Gunther Stent (1978), and George Williams (1989, 1993) have adopted Huxley's Kantian claim that ethics cannot be rooted in human nature because of the unbridgeable gulf between the selfishness of our natural inclinations and the selflessness of our moral duties.[6]

McShea, Masters, and Wilson, however, defend a naturalistic theory of ethics similar to that proposed by Hume and Darwin. They argue that contemporary sociobiological theories for explaining altruistic behavior confirm Darwin's view of sociality and morality as rooted in sympathy, mutuality, and reciprocity. The theory of "inclusive fitness" (Hamilton 1964) explains how natural selection could favor caring not only for ourselves and our offspring but also for our close kin. The theory of "reciprocal altruism" explains how natural selection would favor our helping others and gaining a reputation for being helpful if that increases the probability that others will help us in the future (Alexander 1987; Axelrod 1984; de Waal 1992; Trivers 1971, 1985).

Furthermore, Wilson (1993a, 1993b) argues that natural selection may have promoted a generalized psychological propensity to "attachment" or "affiliation." What he calls "affiliation" corresponds to what Aristotle calls "friendship" (*philia*), a natural drive to social bonding diversely expressed as sexual, familial, companionate, political, or philanthropic attachments (*Ethics* 1155a1–72a15). Wilson believes that the human sentiments of sympathy and benevolence, which throughout most of human evolutionary history would have enhanced reproductive fitness by inclining human parents to care for their young, can now be extended to people who are not one's offspring or even to nonhuman animals. In thus affirming the social dispositions of human beings as rooted in biological nature, Wilson belongs to that growing number of social scientists who argue for going "beyond self-interest" in social theory (Frank 1988; Mansbridge 1990).

Is such naturalistic reasoning about ethics fallacious in moving from *is* to *ought*? If we agree with Kant that the "moral ought" belongs to an utterly autonomous realm of human experience that transcends the natural world, then we would have to say that any move from human nature to human morality is mistaken. But if we agree with Hume that moral obligation is grounded in natural human sentiments or desires, then we would have to say that human morality must be rooted in human nature.

"Value for humans," McShea claims, "arises out of and is validated by their species-specific feeling pattern" (1978, 659). Parenthood is a human value because human beings have a strong feeling for parental caregiving. Friendship is a human value because human beings have a strong feeling for their friends. Courage in war is a human value because human beings have a strong feeling for patriotic loyalty. Such values are natural to human beings, because such feelings arise from what Hume called "the original fabric and formation of the human mind, which is naturally adapted to receive them" (1902, 172). Pure reason alone cannot create values because it cannot create feelings. Reason can, however, elicit, direct, and organize feelings to ensure their fullest satisfaction over a complete life. Indeed, what distinguishes human morality from the behavior of other animals is the cognitive capacity of human beings for reflecting on their present feelings in the light of past experiences and future expectations.

For McShea (1990), the species-typical feelings or desires, which emerged from human evolution to become embedded in the genetic structure of human nature, constitute a universal pattern of motivation for human beings. The good for human beings is the satisfaction of their desires, doing what they feel like doing, doing what they want to do. This is difficult because to do what we want to do, we must know what we really want to do, and then we must know how to get what we want in particular circumstances. Since our natural desires are not reducible to one another, and since they often conflict, their harmonious satisfaction over a whole life requires good habits of choice and prudent judgment. What we ultimately seek in all of our action but attain only with great effort is what Hume called "the state of calm passion," in which each desire is muted to allow for all the others (1888, 417–22, 437, 583; 1902, 239–40).

In this view of morality, ethical naturalists do not commit a "naturalistic fallacy" in moving from *is* to *ought*, McShea explains, as long as they "limit themselves to the assertion that for a particular intelligent species certain feelings are predictably aroused by certain facts and that the experience of such feelings is the only basis on which we can make evaluative judgments" (1990, 226). Again, "Values are prescriptive, imply obligation, because we feel that they do. The fact of obligation is nothing more nor less than the feeling of obligation" (p. 235). Masters endorses McShea's Humean naturalism and sees it as compatible with both Aristotelian and Darwinian naturalism in basing morality on the natural human desires (Masters 1992, 300–302; idem 1993, 115–16). Likewise, Wilson adopts the Humean argument for morality as grounded on sentiment or feeling in developing his own theory of the natural moral sense (1993a, 237–40). Evidence from neurology, behavioral biology, and the social sciences supports this claim that there is a natural sense of justice that arises in the human brain from the interaction of reason and emotion (Damasio 1994; Masters and Gruter 1992).

NATURAL FREEDOM

Kant's primary argument for a radical separation of the natural *is* and the moral *ought*, which would render ethical naturalism indefensible, was that such a separation was a necessary condition for the freedom of the will that must be assumed in all moral judgment (Kant 1965, 409–15, 464–79). For morality to be possible, moral agents must be able to transcend nature through free will. Those who accept Kant's dualism must conclude that biology and the natural sciences in general have nothing to contribute to our understanding of morality, because morality is an utterly autonomous realm that transcends nature (Stent 1978).

In contrast to this Kantian notion of moral freedom as freedom *from* nature, the ethical naturalist would argue that our moral experience requires a notion of moral freedom as freedom *within* nature. For Aristotle, Hume, and Darwin, the uniqueness of human beings as moral agents requires not a free will that transcends nature but a natural capacity to deliberate about one's desires.

Aristotle accounts for moral responsibility with no reference to a free will acting outside the order of nature.[7] He believes we hold people responsible for their actions when they act voluntarily and deliberately (*Ethics* 1109b30–15a3; *Rhetoric* 1368b27–69a7). They act voluntarily when they act knowingly and without external force to satisfy their desires. They act with deliberate choice (*proairesis*) when, having weighed one desire against another in the light of past experience and future expectations, they choose that course of action likely to satisfy their desires harmoniously over a complete life. Such deliberation is required for "virtue in the strict sense," although most human beings most of the time act by impulse and habit with little or no deliberation (*Ethics* 1116b23–17a9, 1144b1–21, 1150a9–16, 1151a11–28, 1179b5–30).

Children and other animals are capable of voluntary action. But only mature human adults have the cognitive capacity for deliberate choice. Thus, for Aristotle, being morally responsible is not being free of one's natural desires. Rather, to be responsible one must organize and manage one's desires through habituation and reflection to conform to some conception of a whole life well lived. One must do this to attain happiness, which is the ultimate end of all human action.

Similarly, Hume believes success in our pursuit of happiness requires a "general calm determination of the passions, founded on some distant view or reflection" (1888, 417–19, 437, 583; idem 1902, 239–40). Rather than acting on the passion of the moment, we can pause while we imagine other passions that we have felt in the past or are likely to feel in the future. Unlike other animals, who act to satisfy whatever desire is stirred by their immediate circumstances, we are moral beings because we can reflect on our desires as experienced over a whole life and weigh what we want now against what we might want in the future. Thus our freedom comes from our ability to delay, to reflect, and then to choose between alternative courses of action based on their distant consequences.

Hume rejects the contrast between free will and determinism as a false dichotomy (1888, 399–412; idem 1902, 80–103). Moral freedom should be identified not as the absence of determinism but as a certain kind of determinism. We are free when our actions are determined by our deliberate choices. In contrast to Kant, Hume doubts that we ever have any real experience of people acting outside the laws of nature. Moral judgment assumes a regular and predictable connection between what people desire and what they do. To hold people responsible for their actions, we must assume that their motives causally determine their actions.

Darwin agrees that "every action whatever is the effect of a motive," and therefore he doubts the existence of free will (1987, 526–27, 536–37, 606–8).

Our motives arise from a complex interaction of innate temperament, individual experience, social learning, and external conditions. Still, although we are not absolutely free of the causal regularities of nature, Darwin believes, we are morally responsible for our actions because of our uniquely human capacity for reflecting on our motives and circumstances and acting in the light of those reflections: "A moral being is one who is capable of reflecting on his past actions and their motives—of approving of some and disapproving of others; and the fact that man is the one being who certainly deserves this designation is the greatest of all distinctions between him and the lower animals" (1936, 912).

Neuroscientists appear close to explaining the neural basis for voluntary action and moral deliberation. Animals with sufficiently large and complex frontal lobes are capable of voluntary action, in the sense that they can learn to adapt their behavior to changing environmental circumstances. Because human beings have larger and more complex frontal lobes than other animals, human beings can use images and words to compare alternative courses of action through mental trial and error and then choose between them, a capacity for rational choice that can be impaired by damage to the frontal lobes (A. Damasio 1994; H. Damasio et al. 1994; Luria 1980, 257–327; Passingham 1993, 1–12, 222–61).

It might seem, however, that such scientific advances in the biological psychology of human action challenge any notion of moral responsibility (E. Wilson 1978, 71–78). For instance, some scientists claim that the inclination to violent criminality is somehow rooted in the neurophysiological constitution of the criminal. This has led some legal scholars to conclude that holding criminals responsible for their crimes is unscientific (Jeffery 1994). If so, this would confirm the common fear that biological explanations of human behavior promote a reductionistic determinism.

Masters and Wilson agree that biological factors influence criminal behavior, because they believe the distinctive character traits of violent criminals—such as impulsiveness and lack of empathy—are to some extent biological (Masters 1994a, 1994b; J. Wilson 1991, 1993a; J. Wilson and Herrnstein 1985). For example, the tendency to certain kinds of impulsive violence seems to be associated with low levels of serotonin (a neurotransmitter).[8] This tendency to violence seems to increase when a deficiency in serotonin is combined with hypoglycemia and alcoholism (Virkkunen et al. 1987, 1989). To the extent that these factors are influenced by genetic endowment, it would seem that some people are naturally more susceptible to criminal violence than others.[9]

Masters and Wilson insist, however, that such biological explanations of human behavior do not deny human freedom. They argue that the biological mechanisms governing human behavior (e.g., the serotonergic system) are too complex to be explained in a simple, reductionistic manner (Tork 1990; Yuwiler, Brammer, and Yuwiler 1994). Genetic propensities interact with individual experience, social learning, and the physical environment. There is some evidence, for example, that social interaction may induce higher levels of serotonin in those individuals who attain high social status (Madsen 1985, 1986; Raleigh and McGuire 1994; Tiger 1992, 248–59).

Alcoholism displays the same causal complexity. Although there probably is some genetic factor influencing alcoholism, the influence is slight in comparison with individual motivation and social circumstances. Some research suggests that people become alcoholics when they are motivated to acquire the habit of drinking alcohol and therefore give up their alcoholism when they are sufficiently motivated to change their habits—although as the habit of heavy drinking becomes stronger, the motivation to change must also become stronger to succeed (Bower 1988a, 1988b; Fingarette 1988a, 1988b). The body of an alcoholic might be abnormal in the way it metabolizes alcohol, but the metabolic process does not itself force the alcoholic to introduce alcohol into his body. The neurophysiological reactions to alcohol occur only after the alcoholic chooses to drink. This sustains Aristotle's claim that alcoholics can be held responsible for their drinking and punished for their conduct while intoxicated, because the *habit* of drinking excessively results from often *choosing* to drink excessively (*Ethics* 1113b22–14a30).

Knowing that some people have a biological propensity to alcoholism or any other disruptive behavior does not lessen their moral responsibility. On the contrary, Masters (1994b) and Wilson (1991) insist, such knowledge enhances their responsibility to control their bad propensities through medical treatment or proper habituation. If we discover that people inclined to low serotonergic functioning and hypoglycemia become dangerously impulsive when they drink alcohol, we could require them to refrain from drinking.[10] If we could identify children who are most at risk for becoming criminals as adults, we could look for ways to alter their childhood environment to promote those moral habits that would protect them against criminality. Knowledge of how good and bad character traits emerge from the complex interaction of nature and nurture enhances our moral freedom.

NATURAL NURTURE

In denying the presumed gaps between facts and values and between nature and freedom, the Darwinian naturalists must also deny the presumed gap between nature and nurture. Many social scientists assume that human biology is largely irrelevant to human social behavior, because they believe that human social life arises from a uniquely human openness to nurture or culture that transcends biological nature. Consequently, the critics of Darwinian explanations of human social behavior insist that "we know of no relevant constraints placed on social processes by human biology" (Allen et al. 1978, 290). Yet if human morality is rooted in natural biological inclinations, and if human freedom is freedom within nature rather than freedom from nature, then the moral freedom that human beings exercise through nurture or culture is biologically natural.

Many social scientists have adopted John Locke's idea that at birth the human mind is a *tabula rasa*, or "white paper," that "receives any characters" (Locke 1959, 1:48, 87, 121). In the absence of innate principles, the mind receives whatever experience and custom write onto it. This would seem to support cultural relativism.[11] Moral principles cannot be naturally innate, Locke argues, if we see that there is hardly any moral norm that has not been violated by some society somewhere in the world. For example, parental care for children might seem to be one moral principle that has been naturally imprinted on the mind. Yet the customary practice of infanticide in some societies shows that even parental care is not a natural moral principle and thus that custom is "a greater power than nature" (Locke 1959, 1:69–88).

Masters argues, however, that Locke is mistaken in his assumption that since the innate or the natural must be universal and invariant, any diversity or flexibility in human behavior must be purely cultural or customary (Masters 1993, 118–29). Modern biology shows that innate traits in most cases are not absolutely fixed, because the observed phenotype emerges from the interaction of inborn potential, developmental history, and the external environment. The human brain and human behavior arise from a complex interplay of natural inclination, individual experience, and social learning (Brauth, Hall, and Dooling 1991; Kandel 1991; Kandel and Jessel 1991). This modern biological account of the interaction of nature and nurture, Masters concludes, conforms more to Aristotle's understanding of human nature than to Locke's view of the mind as a blank slate.

Aristotle believes that what is naturally right varies according to the variable circumstances of particular individuals and particular communities. This does not dictate moral relativism, however, because for any given set of circumstances, there are naturally better and worse ways to satisfy the natural desires of human beings (*Ethics* 1103a14–27, 1107a1–7, 1134b18–35a5, 1137b11–32, 1140a24–45a2; *Politics* 1256a20–b7, 1288b10–89a25; *Rhetoric* 1360b4–66a23; see also Mulhern 1972). Parental care for children, for example, is so deeply rooted in human biology that although its expression will vary according to individual temperament and social conditions, any attempt to abolish parent-child bonding (as suggested in Plato's *Republic*) would be contrary to human nature (*History of Animals* 588b24–89a10; *Ethics* 1161b16–62a28, 1168a22–27; *Politics* 1261b16–62b37).

Similarly, Hume claims that while there is great diversity in the physical world as well as the moral world, in both worlds the diversity is naturally regular: "There is a general course of nature in human actions as well as in the operations of the sun and the climate" (1888, 402). Just as different kinds of trees regularly produce different kinds of fruit in different regions of the world, so do differences in sex, age, and social conditions regularly produce differences in human behavior. If a traveler told us of a country in the Arctic where all the fruits ripen in the winter and wither in the summer, we would not believe him, because this would violate the regularities of physical nature. Nor would we believe him if he told us of a people who lived by the principles of Plato's *Republic* or Hobbes's *Leviathan*, because this would violate "the necessary and uniform principles of human nature" (ibid.).

If one examines carefully the apparent diversity of morals across cultures, Hume contends, one sees that "the principles upon which men reason in morals are always the same, though the conclusions which they draw are often very different" (1902, 335–36). People are often mistaken in their moral reasoning; but since the fundamental principles of morality are uniform, erroneous conclusions can be corrected by better reasoning and wider experience. For instance, in societies like Athens, parents could be motivated by their love of children to commit infanticide if they lacked the resources to care for children properly (1902, 334). Contrary to Locke, Hume believes the practice of infanticide in circumstances where caring for the child is difficult does not deny the naturalness of parental affection (1888, 483–84, 486, 570–73; 1902, 189–92, 201–8, 295–311). Hume would agree with Darwin that parental love and all other moral sentiments are natural to human beings, although their full development requires cultivation by "habit, example, instruction, and reflection" (Darwin 1936, 94).

McShea, Masters, and Wilson agree in rejecting Locke's claim that infanticide shows parental care to be purely a product of culture rather than nature (McShea 1990, 219–22; Masters 1993, 118–29; Wilson 1993a, 18–23). Infanticide tends to occur only in specific circumstances (Daly and Wilson 1988; Eibl-Eibesfeldt 1989, 186–201; Scrimshaw 1984): the infant is deformed, or there is not enough food to feed the infant, or the paternity of the infant is uncertain. Children older than one month are rarely killed, because by then the mother's bond to the child is usually strong. Parents in desperate circumstances may have to kill one infant to preserve another infant or even to preserve themselves. Parental love is natural; but it must compete with other natural sentiments, and this competition forces parents to make tragic choices when their physical and social resources are scarce.

Like human beings, some other animals practice infanticide and abortion in some circumstances (Hausfater and Hrdy 1984). This does not deny the naturalness of parental care for these animals. Rather, it shows the natural variability of animal behavior in adapting to variable circumstances. Behavioral ecology applies Darwinian theory in explaining such behavioral plasticity as biologically natural (Krebs and Davies 1993). Natural selection can favor individual behavioral differences in a species, and these individual differences can be caused either by genetic differences or by mechanisms of phenotypic plasticity such that a single genotype produces behavioral differences in different environments (Kagan 1994; D. Wilson 1994). Thus, as Masters argues, behavioral ecology rejects the assumption of Locke and many social scientists that nature must be invariable and universal (1993, 125–29).

For any organism, the transformation of genotype into phenotype depends upon its unique developmental history of environmental conditions (Lewontin 1992; Oyama 1985). Moreover, animal behavior manifests great phenotypic variability, particularly for those animals with complex nervous systems, so that animals are endowed by nature with the capacity to change their behavior in response to changing circumstances (Gordon 1992; Real 1994). Behavioral ecologists have found that animals in complex societies must weigh social and ecological variables as they make decisions about their lives (Emlen and Wrege 1994; Harcourt and Waal 1992; Masters 1994c). Social intelligence is particularly important for primates (Byrne and Whiten 1988; Seyfarth and Cheney 1994). Some nonhuman primates display, between groups, cultural variation that reflects the unique social history of each group (Dunbar 1988; Goodall 1986; McGrew 1992; Wrangham et al. 1994). So it seems that human beings are not the only cultural animals.[12]

Sociobiology, particularly as developed by E. O. Wilson (1975), has been perceived by its critics as attempting to explain human social behavior as controlled mostly by genetic inheritance, which seems to ignore the complexity and flexibility of human behavior as a purely phenotypic response to variable environments (Kitcher 1985). Behavioral ecology, however, is concerned precisely with such adaptive responses to environmental conditions. Even some of the critics of sociobiology have conceded that human sociobiology would be defensible if it were transformed into human behavioral ecology (Kitcher 1990).[13]

Rather than trying to choose between nature and nurture as the predominant influence on human behavior, the human behavioral ecologist studies the complex interaction of nature and nurture by considering how social and ecological factors influence behavioral flexibility both within and between populations (Borgerhoff Mulder 1991; Crook 1989; Winterhalder and Smith 1992). As a product of Darwinian evolution, human nature should manifest some general propensities that were adaptive in human evolutionary history. Yet the individual expression of those propensities will vary according to individual temperament and in response to the social and physical circumstances of the individual.

Patterns of differential parental investment in children in response to variations in socioecological conditions conform to the predictions of Darwinian theory (Becker 1981, 1991; Voland 1988, 1989). For example, lower-class women in the United States who become teenage mothers might be optimizing the timing of their reproduction for their socioeconomic circumstances, in contrast to middle-class women whose circumstances justify delaying the age of first reproduction. In the conditions of modern industrialized societies, the increasing costs of children for high-status families may favor their investing in fewer children (Lancaster 1994). Thus, parental care for children varies in response to the socioecological conditions of both the parents and the children. This socioecological variability, however, conforms to the regularities of human nature. By contrast, utopian communities attempting to abolish parent-child bonding completely (e.g., the "perfectionists" in Oneida, New York, and the kibbutzim in Israel) discovered that the natural propensity for parental love will eventually assert itself (Klaw 1993; Muncy 1974; Shepher and Tiger 1975; Spiro 1979).

The influence of parenting on children manifests the complex interplay of nature and nurture. Traditionally, many psychologists have assumed that the effects of parental care on children show that environmental socialization is more important than innate temperament in human development. This assumption seems dubious, however, in the face of recent research in behavioral genetics—based largely on adoption and twin studies—that indicates how the natural temperament of the child shapes the social environment. Aristotle and Darwin believe that much of the variability in the mental dispositions of human beings and other animals arises from traits inherited at birth that guide development to adulthood (*Generation of Animals* 767b24–69b31; *Ethics* 1144b1–5; Darwin 1936, 413–15). This seems to be confirmed by the evidence in behavioral genetics that genetic propensities explain much of the variation in human behavioral traits (Bouchard et al. 1990; Plomin, DeFries, and McClearn 1990; Plomin, Owen, and McGuffin 1994). Human variability in personality arises from individual differences in the neurochemical mechanisms that mediate perception and behavior, and to some degree these temperamental differences are inherited (Kagan 1994; Masters 1993, 81–83, 122–23). To a large extent a child's natural propensities foster psychological differences indirectly by influencing the environment of the child. Children with different temperaments evoke different responses from their parents. As children grow older, they have increasing power to modify or select their environments to conform to their individual temperament. Successful parenting is not the imposition of external norms on the child but the cultivation of the child's innate potential. Therefore, we should think of the development of children under parental care as expressing not "nature versus nurture" but "nature through nurture" or the "nature of nurture" (Plomin and Bergeman 1991; Scarr 1992).

The regularity in the patterns of parental care across cultures manifests inborn inclinations shaped by natural selection in evolutionary history. The variability in those patterns manifests the flexibility of human social behavior as shaped by individual experience and social learning. Like other animals, human beings display innate potentialities and propensities that are neither absolutely fixed nor absolutely malleable.[14] For the full development of human beings, nature must be nurtured.

CONCLUSION

The new Darwinian naturalism in political theory challenges the dichotomies that have traditionally separated the social sciences from the natural sciences. There is no absolute gap between *is* and *ought* if human morality is founded on a natural moral sense. There is no absolute gap between nature and freedom if human freedom expresses a natural human capacity for deliberate choice. And there is no absolute gap between nature and nurture if habituation and learning fulfill the natural propensities of human beings. If the new Darwinian naturalists succeed in defending these conclusions, the science of social and political order could become once again—as it was for Aristotle, Hume, and Darwin—the science of human nature.

Notes

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1. Another common objection to ethical naturalism is that it presupposes a teleological view of nature that has been refuted by modern science. In reply, I would argue that although modern biology does not support a cosmic (Platonic) teleology, it does support an immanent (Aristotelian) teleology. For elaboration of this distinction, see Arnhart 1988, 1990 and Lennox 1992, 1993.

2. Hume suggests that his science of human nature could be rooted ultimately in biological sciences such as anatomy and physiology (1888, 7, 13, 176–79, 190, 212, 248, 275–76, 324–28, 340–41; idem 1902, 10, 104–8). Some recent research in neurobiology seems to confirm Hume's argument that reason and emotion are distinct and yet complementary causes of human behavior, because rational conduct must be guided by emotional assessments that enforce a system of preferences. If this is so, then Kant's conception of ethical rationality as utterly free from emotion is impossible (Damasio 1994; McShea 1978, 665–66; Masters 1993, 130–34). As Aristotle saw, the emotions are essential to good reasoning because they contain judgments about the world as it relates to our lives (Arnhart 1981, 111–34).

3. The translations of Aristotle's Greek are mine, although I have consulted the translations in *The Complete Works of Aristotle* as edited by Jonathan Barnes (Aristotle 1984).

4. Aristotle's remarks about the primacy of desire in motivating moral action were often cited with approval by the moral-sense philosophers (e.g., Barratt 1869, 20, 97, 192, 197).

5. Contrary to the assumption of some feminists that Aristotle's biological naturalism denigrates women, his argument for maternal caregiving as the natural root of human sociality elevates the claims of women (Arnhart 1992, 1994b).

6. This Kantian separation between natural selfishness and moral selflessness shows the influence of Augustinian asceticism. For Augustine, no act is truly virtuous if it is rooted in self-love, but since self-love is natural to human beings in their fallen state, true virtue requires a transcendence of nature through grace (*City of God* 5. 12–13, 20; 8. 8; 14. 13, 28). By contrast, Aristotle believed that since the final end of ethics is happiness, living virtuously expresses one's natural self-love (*Ethics* 1168a28–69b2). From Aristotle's perspective, the Kantian view of morality as utterly selfless makes it hard to see what motive anyone would have to be moral. Thomas Aquinas follows Aristotle in teaching that by nature, our love of others is an extension of our self-love, and thus we should love those nearest to us—our family, friends, and fellow citizens—more than strangers (*Summa theologica* II-II, q. 26, aa. 4–8; q. 31, a. 3). Aristotle and Aquinas would disagree with those sociobiological theorists who identify morality with altruism and then define altruism as selfless behavior (Chandler 1991; Dawkins 1976, 215; Wilson 1975, 578).

7. Some scholars believe the idea of free will was not even discussed by anyone until Augustine first formulated it as a tenet of his theology (Arendt 1978; Augustine 1964), although some early Christian theologians interpreted Aristotle's *proairesis* as free will (Pelikan 1993, 129–31, 144, 159–60, 283, 323). In Kant's commentary on the biblical account of Creation in the book of Genesis, he developed the modern concept of culture as a uniquely human expression of freedom from nature that manifests the moral dignity of human beings as created "in the image of God" (Arnhart 1994a; Kant 1983). Contemporary creationist critics of Darwinian biology advance the same dualistic claim for human culture as transcending biological nature (Morris 1985, 178–208). Unlike Kant and the creationists, Darwinian naturalists such as McShea and Wilson believe that human morality has a purely natural basis (McShea 1990, 49–53; Wilson 1993a, 220–21). Masters seems more open to the claims of religious faith (1993, 6–11, 152–53). If Kant's notion of freedom as transcending nature is a postulate of his religious faith and is therefore part of his project "to deny *knowledge*, in order to make room for *faith*," it would seem that such an idea cannot be derived from natural reason and natural human experience without the aid of divine revelation (Kant 1965, 29).

8. One likely explanation for the success of Prozac (fluoxetine) as a popular antidepressant drug is that it raises the level of serotonin (Jacobs 1994; Kramer 1993).

9. Modern research on the psychology of criminal behavior as surveyed by Wilson and Herrnstein (1985) seems to confirm Aristotle's insights (*Ethics* 1148b15–49a20; *Rhetoric* 1372a4–73a40).

10. Even so resolute a libertarian as John Stuart Mill would support this: "The making himself drunk, in a person whom drunkenness excites to do harm to others, is a crime against others" (1956, 119).

11. Locke himself, however, was not a cultural relativist, because he thought some moral principles could be rooted in human nature (Grant 1987, 38–51).

12. I agree with Gary Johnson (1995) that if we define government as "centralized coordination of a social system" and politics as "competition over the conduct of government," then government and politics can be found in the life of every sexually-reproducing social species—from ants to chimpanzees to human beings. This would confirm Aristotle's claim that human beings are not the only political animals (Arnhart 1994a).

13. Some social scientists assume that sociobiology has been refuted by biological critics such as Stephen Jay Gould and Richard Lewontin. The flaws in the argumentation of Gould and Lewontin are so serious, however, that their work is now studied as a model of sophistical rhetoric in science (Bazerman 1993; Borgia 1994; Charney 1993; Gould and Lewontin 1979).

14. Both Masters and Wilson view the collapse of Marxist socialism in the Soviet Union as historical evidence against the cultural determinist belief that human beings are infinitely malleable through social learning (Masters 1993, 94, 118–19, 135, 198–200; Wilson 1993a, 232, 251). Similarly, a Chinese scholar has argued that the failure of Maoist socialism confirms the sociobiological theory of human nature (Zhang Boshu 1994).

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THOMAS HOBBES: SKEPTICAL MORALIST

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Thomas Hobbes is usually held to have been a skeptic in matters of religion and morality. I accept the claim that there is a distinctive skeptical strain in Hobbes' thought but argue that his skepticism informs his moral vision, rather than depriving him of a conception of morality. As evidence for this reading, I situate Hobbes in a tradition of "skeptical moralism," along with Montaigne and certain other Renaissance figures. As opposed to moral skeptics, skeptical moralists think of moral agents as divided selves, pulled in one direction by law and another by conscience. Skeptical moralists use skepticism to make people aware of this tension, and I argue that (especially in his remarks on religion) Hobbes was doing just that.

To most political theorists, the claim that Thomas Hobbes was a skeptic in matters of religion and ethics would come as no surprise. That Hobbes doubted the occurrence of miracles, the possibility of divine inspiration, the existence of prophets, and the authenticity of Scripture has been well documented. In matters of religion, he believed, we have no choice but to accept the judgment of the sovereign as authoritative—not because the sovereign has any better knowledge than we do about what God requires of us but because *no one* has any reliable knowledge about that. Similarly, we must rely on the sovereign to declare "a common standard for virtues and vices" (1978, 69), because we cannot rationally justify any of our moral beliefs or communicate them to others. Take heed of words, Hobbes warned, especially when they are used to express an evaluation of someone's character or conduct; for "besides the signification of what we imagine of their nature," these words have "a signification also of the nature, disposition, and interest of the speaker." Thus "one man calleth *Wisdome,* what another calleth *feare*; and one *cruelty,* what another *justice,*" and so on (1968, 109). As in religion, so in ethics: we have no choice but to accept the sovereign's judgment, because our capacity for speech and reason is not sufficient to enable us to settle our disagreements on our own.

There is another claim—different from, but related to, the claim that Hobbes was a skeptic in matters of religion and ethics—that has gained widespread acceptance. This holds that genuine moral and religious considerations play no significant part in Hobbes' political theory. Thus, Hobbes' seventeenth-century critics expressed the suspicion that his religious skepticism was merely a disguised form of atheism. "To bottom all Religion upon humane authority," one such critic complained, "and derive it from the power and pleasure of men, tends . . . to destroy all Religion, and at last to bring men to no Religion" (Sir Charles Wolesley, quoted in Miutz 1969, 53). "God help us," lamented Bishop Bramhall, "into what times are we fallen, when the immutable laws of God and nature are made to depend upon the mutable laws of mortal men, just as one should go about to control the sun by the authority of the clock" (quoted in Hobbes 1840, 4:372–73).

Twentieth-century commentators have not been especially bothered by Hobbes' apparent atheism (though most of them do not dispute the charge). Their imaginations have been captured instead by his apparent utilitarianism and general amoralism. Genuine moral obligation, Thomas Nagel has argued, "plays no part in *Leviathan* at all"; what Hobbes called moral obligation, he goes on to say, "is based exclusively on considerations of rational self-interest" (Nagel 1959, 69). Likewise, John Watkins sees Hobbes as having wanted "to reduce morality to rational self-interest" (Watkins 1989, 57). Even Richard Tuck, who dismisses as anachronistic the view that Hobbes had a utilitarian or prudentialist conception of human action, concludes that his moral theory amounted to little more than the claim that people have a right to do what they must to preserve themselves. Hobbes, he says, "was left with little more to stand on as a guide to living than the sceptics like Montaigne, for they too had always acknowledged the practical force of the principle of self-preservation" (Tuck 1989, 115).

One is left with the impression that Hobbes' skepticism led him to conclude that a genuine moral or religious life is not possible for human beings, or at least that such a life is not possible for the subjects of a Leviathan state. I shall dispute this widespread view. I want to argue, in other words, that Hobbes did offer a genuine moral philosophy. This, of course, is not the first attempt to make such an argument. Others (most notably A. E. Taylor and Howard Warrender) have sought to show that Hobbes held fairly traditional moral and religious views and that these informed his theory of political obligation. My argument differs from theirs, however, in that I do want to take seriously the undeniable elements of moral and religious skepticism in Hobbes' writings. Indeed, my central contention is that Hobbes used skeptical argument as a foundation upon which to construct a challenging (if not terribly attractive) conception of moral life. This will lead me to conclude that while Hobbes was not a *moral skeptic* (using skepticism to subvert moral and religious sensibility), he was a *skeptical moralist* (allowing skepticism to inform his vision of a good life for human beings).

THOMAS HOBBES: MORAL SKEPTIC?

Let me begin by saying more about the difference between a moral skeptic and a skeptical moralist. Both doubt that we can (at least in many circumstances) tell the difference between genuine and pretended virtue. The moral skeptic goes on to question the point of differentiating between what is *really* good and that which *appears* good. If there are no neutral or objective standards of moral judgment and if all I have to go on in matters of morality is my own judgment, then what I judge to be good is good for me, although it may not be for you. I may decide later that the judgment I made a while ago was in error; but this subsequent judgment is itself relative to a standard which is not neutral and which cannot be said to be superior to other such standards. To the moral skeptic, then, moral argument is all a matter of appearances; there is no point in distinguishing between what seems good and what is good.

A skeptical moralist, by contrast, maintains that the distinction between genuine and pretended virtue is still worth making. The fact that in many cases, at least, we find it a difficult distinction to draw is a circumstance skeptical moralists exploit to get us to think more deeply and self-critically about moral issues. In raising doubts about our competence to exercise moral judgment, they criticize from within, unlike the moral skeptic, whose criticism calls into question the very practice of exercising thoughtful judgment in matters of morality. Montaigne is an example of a skeptical moralist. The teachings of classical skepticism are important, he said, because they portray "Man naked, empty, aware of his natural weakness, fit to accept outside help from on high: Man, stripped of all human learning and so all the more able to lodge the divine within him, annihilating his intellect to make room for faith" (1987, 74).[1] Skepticism in this sense does not teach us to disregard standards of moral judgment simply because they are all relative. The aim of the skeptical moralist is to nurture an awareness of our fallibility, using that as a means to inculcate the virtues that fallible humans ought to exhibit. Montaigne's description of the Pyrrhonian skeptic furnishes a catalog of these virtues. The skeptic, he said, is "humble, obedient, teachable, keen to learn. . . . He is a blank writing-tablet, made ready for the finger of God to carve such letters on him as he pleases" (Ibid.).

Given the choice, most readers would probably classify Hobbes as a moral skeptic, rather than a skeptical moralist. In each of his major works on politics, Hobbes offered a causal analysis of the passions to explain the apparent diversity of moral beliefs and customs. "Such is the nature of man," he observed in *De cive*, "that every one calls that *good* which he desires, and *evil* which he eschews" (1978, 282). One finds the same idea in *Leviathan*, expressed in almost identical language: "But whatsoever is the object of any mans Appetite or Desire; that is it, which he for his part calleth *Good*" (1968, 120; see also 1840, 4:31–32). It is because appetites and desires differ that "one counts that *good*, which another counts *evil*; and the same man what now he esteemed for *good*, he immediately after looks on as *evil*: and the same thing which he calls *good* in himself, he terms *evil* in another" (1978, 282–83). In passages such as these, where differences in moral judgment are resolved into "the diversity of affections" (Ibid.), Hobbes does indeed give the appearance of having been a moral skeptic. One is left with the impression that he regarded virtuous conduct with indifference. It is as though respect for others, in his judgment, serves only to gratify some private inclination, and that as Hume observed in his critique of Hobbism, "an inconsiderable turn of thought forms the whole difference" between a generous and a miserly disposition (1975, 297).

There is, however, another aspect of Hobbes' thought with which this interpretation is not easily reconciled. When writing as a scientist of the passions (as he did, e.g., in *Leviathan* 6), Hobbes seems to have embraced the view that moral language has a subjective character, that it is suitable only for the expression of private appetites. Two chapters later, however, Hobbes turned his attention to "the Vertues commonly called Intellectual; and their contrary Defects" (or vices, 1968, 134). In fact, the discussion of this chapter focuses mainly on one intellectual virtue: judgment, or discretion, and the absence of this virtue (or its defect), madness. Furthermore, 'judgment' has a somewhat narrower meaning in Hobbes' theory than it usually does in more traditional ethical theories (e.g., Aristotle's). Judgment, for Hobbes, meant the ability to exercise self-restraint. People who lack this virtue, he said, are like those who have drunk too much wine. Just as wine "does but remove Dissimulation; and take from them the sight of the deformity of their Passions," so the indiscreet person is the victim of his or her "extravagant" passions, which are "for the most part meere Madnesse" (1968, 142).

What I find ironic and rather troubling about this discussion, in light of the subjectivist account of moral language that has preceded it, is Hobbes' own appropriation of a nonexpressivist moral vocabulary. While the concept of judgment takes on a somewhat unusual meaning in his theory, his characterization of it as an intellectual virtue is quite traditional and not in keeping with the apparent moral skepticism of earlier chapters. In chapter 5, Hobbes had warned that language invariably reflects "the nature, disposition, and interest of the speaker" and that this is especially true with respect to "the names of Vertues and Vices" (1968, 109). In chapter 8, however, "Intellectual Vertue" is used to commend a particular disposition, or "good witte" (ibid. 134). It follows that what counts as a virtuous intellect or good wit does not depend on the disposition of the speaker. Hobbes was telling his readers that without "Steddinesse, and Direction to some End" the speaker, whatever he may think of his own disposition, is giddy, distracted, or mad (ibid. 136, 139). This suggests that there is at least one kind of judgment—"good witte"—for which a neutral and objective criterion of excellence exists and therefore that there is some point to distinguishing between judgment that is really good and that which (wrongly) appears good—(to someone, say, in the grip of madness or (what amounts to the same thing) enthusiasm).

If we begin with the premise that Hobbes was a moral skeptic, then the discussion of intellectual virtue in chapter 8 of *Leviathan* represents an anomaly.[2] Hobbes' characterization of his laws of nature as moral laws and of the dispositions they prescribe as "good manners or habits, that is, virtues" (1978, 151), also leaves the reader with this impression. The first two chapters of *De cive* portray the predicament in which Hobbesian individuals find themselves, when each must rely on his or her private judgment to decide whether the acts of others pose a threat and how to respond if they do. The state of nature has been described (aptly, in my view) as "a state of subjectivity" (Wolin 1960, 258). Life in this chaotic condition reflects the apparent skepticism in Hobbes' emotivist account of moral language. In the absence of "a common standard for virtues and vices" (1978, 69), this is what life would be like. Once again, however, this analysis (which seems to be the work of a convinced moral skeptic) is followed by a decidedly nonsubjectivist discourse about virtue, in this case moral (or civic) virtue.

Chapter 3 of *De cive*, most of which is devoted to elaborating some 20 laws of nature, concludes with a consideration of the "dispositions of the mind" exhibited by "a just man."[3] A just man is one "who tends to this with his whole might, namely, that his actions be squared according to the precepts of nature" (1978, 149, 150). The conduct of such a person may not always be virtuous, or in accord with the precepts of nature; that will depend in part upon how this person is treated by others. For the law of nature does not require that one disregard one's physical security or overlook the threatening conduct of others. But the just person will be disposed or inclined to peace; he or she will have acquired the "good manners or habits, that is virtues" which are necessary for peace (e.g., modesty, equity, trust, humanity, and mercy), as enjoined by the law of nature (ibid. 151). As in the case of the intellectual virtues, these moral attributes were defined more narrowly by Hobbes than by most other philosophers. In explicit contrast to Aristotle, Hobbes insisted that the precepts of nature can be considered *virtues* only insofar as they serve the cause of peace; and, of course, in Hobbes' political theory, the difference between peace and subservience is rather vague.[4] Still, Hobbes' account of the moral virtues (like his analysis of intellectual virtue) is in striking contrast to the subjectivist tone of his moral psychology. Unlike the names 'good' and 'evil'—"that signifie our Appetites, and Aversions; which in different tempers, customes, and doctrines of men, are different" (1968, 216)—conduct that counts as just or virtuous is fixed by objective standards given in the law of nature, "the indubitable everlasting Law of God" (ibid. 529).

There is no reason to think that Hobbes' account of the virtues should be of less importance to his political theory than his causal analysis of the passions. But as we have seen, it is difficult to reconcile his more traditional-sounding remarks about virtue with the relativist conclusions that follow from his analysis of the passions. Having shown that the language we use to talk about virtue and vice is an unreliable means of communication, Hobbes did not dismiss it, as he should have if he had been a consistent moral skeptic. Instead, he thought it necessary to furnish his own account of the virtues. After discrediting our use of moral language, he used this language himself. Why?

The tension between Hobbes' skepticism and his moralism has not received the attention it deserves. Hobbes' skepticism has been carefully and thoughtfully analyzed by Richard Tuck, whose work has illuminated the connection between Hobbes and the skepticism of Renaissance humanists. Hobbes' moralism has been given an imaginative reading by Mary Dietz. Taking her cue from civic republicanism, she argues that Hobbes was a civic moralist, who sought to "reconstitute" subjects as citizens, instilling in them civic qualities or virtues from which a sense of duty to the commonwealth would spring naturally (Dietz 1990, 107). As important as their work is, neither Tuck nor Dietz explores the tension between skepticism and moralism in Hobbes' theory. Tuck contends that like his skeptical predecessors, Hobbes offered a "radically thinned down" conception of moral life, in which there is little room for the elaborate account of virtue that one finds, for example, in Aristotle (Tuck 1989, 21; idem 1993, 175). Dietz's essay, on the other hand, is mainly concerned with the practical, "civic education" side of Hobbes' theory. Thus she focuses on the more political parts of his work and does not discuss the skeptical aspects of his theory of human nature.[5]

I shall bring these two sides of Hobbes' theory together. To support my claim that skepticism informed Hobbes' vision of a good life for human beings, I need to say more about what this vision was both for Hobbes and for skeptical moralists generally. Does the tradition of skeptical moralism give us a way to think about virtuous conduct, or moral agency? If so, what is it, and what (if anything) can we derive from it to better understand the idea of virtue in Hobbes' theory?

DIVIDED SELVES: SKEPTICISM AND MORAL LIFE

"Every subject's duty is the king's," wrote Shakespeare in *Henry V*, "but every subject's soul is his own" (4.1). This could have been Hobbes' motto as well, at least in part 3 of *Leviathan*, where liberty of conscience is given a consistent (if qualified) defense. While granting to kings and princes "all manner of Power over . . . mens externall actions, both in Policy, and Religion," Hobbes insisted that this power does not extend to religious faith or belief, of which "there is no Judg at all, but God, that knoweth the heart" (1968, 575–76). Faith is "internall"; it is "a gift of God, which Man can neither give, nor take away by promises of rewards, or menaces of torture" (ibid. 527; 550). The sovereign may compel me to act in ways I otherwise would not or to profess things I do not believe; and when he does, I must obey. But he may not oblige me "to think any otherwise then my reason perswades me" (ibid. 411).

Subjects have inner lives, Hobbes believed; and inner life is off-limits to the sovereign, if only because the sovereign lacks the techniques that would enable him to reach within and condition people into submission. I am not suggesting (as some have) that Hobbes was a defender of the principle of toleration,[6] for despite their liberty to think as they like, subjects clearly are not at liberty to express their thoughts publicly, if they run counter to authorized doctrine. But the position of a subject is even more precarious than I have so far indicated. Not only must they bring their conduct (including speech) into conformity with the law; subjects must also strive to *internalize* the law's precepts, making them the rule of their actions in fact, not just in appearance. The law, Hobbes said, is "the Publique Conscience" (1968, 366); thus, as a subject, one "doth according to his conscience and judgment, as having deposited his judgment in all controversies in the hands of the sovereign power" (1840, 4:70). We are obligated to submit our wills and our judgments to the will and judgment of the sovereign, so that the commonwealth may become in fact what it is in principle, "a reall Unitie" of all who compose it (1968, 227).

To be a subject, then, is to find oneself in a difficult predicament. Not only are the subject's thoughts likely to be at odds with his or her speech and conduct, but subjects may be divided internally as well, pulled in one direction by the law and in another direction by conscience. The potential for conflict seems to have been built into the subject's psyche. Cognitive dissonance seems to have been part of Hobbes' design. Of course, it is possible that Hobbes was simply being careless, in which case we need not take seriously the idea that subjects have inner lives. Or, as Alan Ryan has suggested, it may be that while subjects have been outfitted with inner lives, no moral significance attaches to this. That subjects have inner lives may signify little more than that seventeenth-century sovereigns were not as well equipped as twentieth-century sovereigns are to manipulate subjects' minds: "The sovereign cannot condition children as the Director in *Brave New World* can, and therefore should not try. There is no evident reason of principle to stop him applying the techniques when they are discovered" (Ryan 1983, 217).

It seems to me, however, that Hobbes' subject is a deeply moral figure and that the subject's divided psyche is of considerable moral significance. I realize that this characterization of the subject will seem odd. Properly speaking, to be a moral agent one must possess a certain degree of autonomy; one must be able to judge for oneself and to act in accordance with one's judgment. But Hobbes' subjects appear to lack the required degree of autonomy. Even if they could judge for themselves, they are not in a position to act as their judgment dictates. And since they are subject to conflicting imperatives, whether they are really in a position to judge for themselves seems questionable. In what sense, then, can Hobbes' subject be called a moral agent?

While the view of moral agency I am attributing to Hobbes may seem odd to us, it was not such an uncommon view in his own day. The conflicted psyche I have been discussing appears with some frequency in works by Renaissance skeptics, among them Pierre Charron. One of the dispositions essential to moral wisdom, Charron said, is "an entire Liberty of the Mind," which consists in "considering, judging, and examining all things; yet not Tying ones self up to any, but remaining still free." And yet, as he went on to observe, those who possess this liberty of mind will frequently find themselves at odds with themselves:

> Now I own, that while a Wise Man asserts this liberty to himself, it cannot be expected, that he shall be always of a piece: For at this rate his Hand and his Opinion, his Body and his Mind will be frequently put upon contradictions to one another; and there is no avoiding it: because Prudence governs his outward Actions, and private Judgment the Sentiments of his Soul. So that here are two different Characters to be maintained, and he must play both, or be unjust either to the World, or to himself. (1697; 2.16, 23)

By virtue of his wisdom, Charron's wise man is in the same predicament as Hobbes' subject. Neither is allowed to make the standard of his judgment the rule of his actions. Yet Charron purported to be describing a person with a conscience—a moral agent.

The passages I have quoted come from Charron's *Of Wisdom*, a highly popular work with which Hobbes would undoubtedly have been familiar.[7] Charron is generally regarded as an unoriginal writer, whose ideas were drawn mainly from Montaigne's *Essays*. Indeed, one finds in Montaigne the same image of the moral agent as a figure at war with itself.[8] And it is through his *Essays*, I believe, that we are best able to understand the moral significance of this image.

The ceaseless bloodletting over religion he had witnessed during his lifetime was never far beneath the surface of Montaigne's writing. His best-known essay, *An Apology for Raymond Sebond*, opens with the observation that religion, which ought to serve as a check on our passions, now "cloaks them, nurses them, stimulates them" (1987, 7). Partisans for and against the Roman Church, Montaigne lamented, had come to resemble no one more than each other. Seduced by the rhetoric of moral purity, neither side could any longer evaluate the fairness of its conduct or the sincerity of its motives. Viciousness donned the cloak of righteousness. Ambition had become courage, stubbornness masqueraded as piety, and behind every diplomatic overture there lurked a blasphemous heart. Montaigne feared that language itself was in danger of falling victim to the angry passions unleashed by war: "We are experiencing what Thucydides says of the civil wars of his time, that men baptized public vices with new milder names to excuse them, adulterating and softening their true titles" (1957, 87). The ability to unmask dissimulation, even in one's own conduct, had become a casualty of war.

Scholars are generally agreed that Montaigne's skepticism was a response to this predicament. There is less agreement, though, on the nature of his skepticism. I read Montaigne as a skeptical moralist, who used skepticism to refine and civilize judgment, not to get us to "renounce" our judgment and "purge" ourselves of potentially disruptive beliefs, as Tuck argues (1993, xvii). To judge fairly, the skeptical moralist holds, we must be able to subject ourselves to self-critical scrutiny; and that, in turn, requires an attitude of respect for the laws and customs of one's country. It will be useful to explore the development of each of these points in Montaigne's *Essays*, since (as I shall argue) both principles are at work in Hobbes' theory as well.

In a brief but lively essay ("Of Cannibals") Montaigne described the way of life of a Brazilian tribe whose customs, in many respects, resembled those of his own countrymen. Although lacking a written language and a formal system of laws, they believed in the immortality of souls, worshipped together in public places, composed and recited poetry, and took pleasure in many of the same activities sixteenth-century Europeans did—with one striking exception: prisoners taken in war, after being put to death, were ceremonially roasted and eaten. What, Montaigne asked, are we to make of a people that permits such savagery? "I think there is nothing barbarous and savage in that nation, from what I have been told, except that each man calls barbarism whatever is not his own practice." Reminding his readers of the punishments routinely meted out in the prisons of "civilized" nations, he added, "There is more barbarity . . . in tearing by tortures and the rack a body still full of feeling, in roasting a man bit by bit [while still alive] . . . than in roasting and eating him after he is dead" (1957, 152, 155).

Pushed to an extreme, the relativist sentiments expressed here would make the responsible use of language (and the unmasking of pretense) impossible; for if I am permitted to call barbarism whatever is not consistent with my practice, then 'barbarism' can mean whatever I want it to mean. But this is not the message Montaigne was trying to convey. On the contrary, his purpose was to disclose the pretense in *our own* use of moral language. In referring to the condition of prisoners in so-called "civilized" nations, Montaigne was not *defending* his country's treatment of them. He was not saying, "What we do to them may seem cruel, but at least we don't eat the poor wretches, as some people do!" He was saying that no nation has a monopoly on civilization (or barbarism)—that *civilization*, properly understood, is a matter of being able to detach oneself from one's own customs and judge them impartially, not of preferring one set of customs to another. Self-criticism, Montaigne thought, is essential both to the proper use of language and to moral agency. The ability to criticize the practices of others is less important.

Self-criticism can be pushed too far, however, and then it becomes debilitating. Speaking of the decades of turbulence caused by efforts to reform the Catholic Church, Montaigne complained, "I am disgusted with innovation, in whatever guise, and with reason, for I have seen very harmful effects of it" (1957, 86). He was not opposed to change as such; nor was he against all forms of political involvement, despite his preference for private life. Commenting on his own years of public service, he observed that "it would be a sort of treason" to disengage entirely from the controversy surrounding religion and reform of the Church (1957, 601).[9] But, Montaigne insisted, intelligent criticism is criticism that knows its limit. Describing the mind as a "dangerous tool," he observed that when it is "allowed to deviate, however slightly, from the normal path, turning and straying from the beaten track traced for us by the Church," we stumble about with no sense of direction. And whatever their intentions, those who attack the Church succeed only in undermining "that great public highway" that helps to keep criticism on course and within intelligible limits (1987, 90–91, 137).

The willingness to detach oneself from one's prejudices and the customs of one's country while respecting the limits of detached judgment is the essence of Montaigne's skepticism. Montaigne was not a moral skeptic. He set out not to demonstrate that "independent judgment must destroy itself" (Tuck 1993, 307) but to instill in his readers an appreciation of the fragility of judgment and its susceptibility to abuse. Again, the implication is not that we should renounce the use of our judgment but that in using it, we should strive to keep it within proper bounds. For these reasons, I think it is fair to consider Montaigne a skeptical moralist.

The experience of being in conflict or at odds with oneself is at least one common thread linking Hobbes with the Renaissance skeptics.[10] Hobbes' subject, I said, is pulled in one direction by law and another by conscience. Montaigne's *Essays* portray the moral agent in a similar predicament. Driven by our wariness of dissimulation, we seek a measure of detachment only to discover that the detached standpoint we seek is an illusory goal. We are left to struggle for ourselves with the task of striking a balance between these two conflicting aims. But is there more than a superficial resemblance between Hobbes' subject and Montaigne's skeptical moralist? Montaigne was clearly struggling to attain a measure of clarity about the demands of moral life. Is there any evidence to suggest that Hobbes was concerned about this? Why assume, in other words, that the problem of a divided psyche is, for Hobbes' subject, a *moral* problem?

TEACHING THE ART OF SELF-DOUBT: HOBBES' MORAL SUBJECTS

Conflict over religion was as much Hobbes' concern as it had been Montaigne's. Like the Renaissance skeptics, Hobbes worried about the deterioration of moral speech during countless years of civil war. Faith, duty, conscience, and other high-minded ideals had been deployed by every sect and faction to conceal its pursuit of narrow, partisan ends. As a result, respect for these ideals—and the public standards on which coherence in moral life depends—had diminished.

The behavior of Presbyterian ministers during the years leading up to the English Civil War provided Hobbes with a concrete illustration of the way in which moral speech lends itself to abuse. Referring to the ministers' style of preaching, he observed:

> No tragedian in the world could have acted the part of a right godly man better than these did; insomuch as a man unacquainted with such art, could never suspect any ambitious plot in them to raise sedition against the state, as they then had designed; or doubt that the vehemence of their voice (for the same words with the usual pronunciation had been of little force) and forcedness of their gesture and looks, could arise from anything else but zeal to the service of God. (1990, 24)

It is clear enough from these remarks that Hobbes regarded Presbyterian preaching as a fraud. The ministers' physical appearance was pure dissimulation, their manner of speaking an artful disguise, their zeal an affectation. Like consummate actors, they feigned piety, pretending that the words they spoke were not theirs but God's. In Hobbes' view, they could not have concealed their seditious intent from a credulous multitude more cleverly.

It is equally clear, however, that these rebels were not out simply to enrich themselves or to enhance their own power and reputation. Ambition, in this case, is not analogous to the ambition of a crafty politician, who cynically manipulates and knowingly lies to his or her constituency for the sake of personal gain. If the Presbyterian ministers were indeed perpetrators of deception, they were also its victims. Hobbes recognized this. He conceded that in opposing the king, the ministers were acting in their capacity as Christians, doing what they (erroneously) believed duty required of them. Ideology, rather than naked self-interest, lay at the root of the rebellion: "They will say they did it in obedience to God, inasmuch as they did believe it was according to the Scripture; out of which they will bring examples, perhaps of David and his adherents, that resisted King Saul, and of the prophets afterward, that vehemently from time to time preached against the idolatrous Kings of Israel and Judah" (1990, 49). In their own defense, the ministers could call upon a doctrine that even the king's adherents would have had no trouble accepting: that "when [the king] enjoins anything contrary to what God hath commanded, . . . we are, in that case, to obey God rather than men" (ibid.).[11] This doctrine, Hobbes said, "divides a kingdom within itself," for it invites each person to judge the lawfulness of what the king has enjoined (ibid., 50).

Presumably, though, the ministers thought their preaching agreeable to God's will, as revealed in Scripture. If they thought so, it cannot be said that they were acting without conviction, putting interest or personal ambition ahead of duty.[12] But in that case, how can their action be portrayed as fraudulent or deceptive? Their zeal, it would seem, was anything but affected; their intentions were pious, their godliness sincere. What, exactly, was the nature of their supposed misconduct? In what sense was their preaching an *abuse* of moral or religious speech?

Let me pause here to take note of the character of Hobbes' writing in these passages. The imagery is vivid, the sarcasm biting. These fanatical preachers all but leap off the page, with their sanctimonious grimacing, their hectoring voices, their pained expressions of righteous indignation—a fine example indeed of polemical writing. Hobbes seems to have wanted the reader to feel the weight of the dilemma that, in his view, Presbyterian preaching had posed to all of England—namely, How is one to distinguish between genuine piety and pious imposture, between the proper use of "that reverenced name of Conscience" and the misuse of it by "men, vehemently in love with their own opinions, (though never so absurd)" (1968, 132)? His intent, I suspect, was to make his readers feel helpless in the face of this difficulty, to make the fragility of their judgment palpable to them. In this respect, at least, Hobbes' writing in these passages bears a clear resemblance to that of Montaigne in his essay on cannibals. Both writers, it seems, wanted to draw their readers inside a moral predicament.

Like Montaigne in the aforementioned essay, Hobbes went on to draw the moral of his lesson: "If it be lawful then for subjects to resist the King, when he commands anything that is against the Scripture, . . . it is impossible that the life of any King, or the peace of any Christian kingdom, can be long secure" (1990, 50). In their haste to condemn what they took to be an objectionable form of worship, the dissentient ministers acted unreflectively; they overlooked the larger implications of their action. Having undermined the standards officially authorized in civil law, where does one turn to discern the "true sense" of Scripture? To the dissenters' creed? Perhaps their reading merely reflects their own bias; "and then all this stubbornness and contumacy towards the King and his laws, is nothing but pride of heart and ambition, or else imposture" (ibid. 53).

But the moral of this lesson is not nearly as important as the manner of its delivery. Put another way, the moral is that in matters of religion we should always feel doubtful, hesitant, unsure of ourselves. The essence of (Christian) religion, Hobbes said, is "a quiet waiting for the coming again of our blessed Saviour, and in the mean time a resolution to obey the King's laws (which also are God's laws)" (1990, 58). But he did not present this as the conclusion of an argument, deduced logically from indubitable premises. Rather, he sought to instill a certain disposition, a skeptical mental outlook, from which the appropriate responses (doubt, hesitancy, uncertainty) spring naturally and of their own accord.[13] He did so, moreover, not by lecturing but by confronting his readers with a moral predicament. He argued as an internal critic—not against religion but with it. In *Behemoth*, at least, Hobbes was a skeptical moralist, not a moral skeptic.

Not only in *Behemoth*, I contend, but in *Leviathan* as well. In its treatment of religion, *Leviathan* is a notoriously ambiguous text. Consider, for example, the discussion of miracles in chapter 37. Miracles, Hobbes said, are "Marvellous workes" (1968, 332), but what is marvelous to one may not be so to another. "The first Rainbow that was seen in the world, was a Miracle." Why? Because it was the first—because people had had no prior experience of rainbows (and, Hobbes added, because of the circumstances in which it appeared, namely, after the great Flood described in Genesis, when it was taken as a sign from God that he would torment his people no longer). But nowadays, rainbows are not miracles. On the other hand, "if a Horse, or Cow should speak, it were a Miracle" (ibid. 470). In short, what is miraculous and what is not depends on the knowledge and experience of the observer. This, of course, is not to deny that miracles have occurred or that they can occur. It does imply, though, that what is called a miracle is usually nothing more than an act of deception, "which is no Miracle, but a very easie matter to doe" (ibid. 475).

What are we to make of this? The generally accepted view is that Hobbes was trying to refashion Christianity to fit more easily with his rationalistic theory and that his debunking of miracles was part of this refashioning. David Johnston has given a clever twist to this reading. *Leviathan*, he argues, is a polemical work, not (as is commonly assumed) a philosophical treatise. Its aim is not so much to inform as to *transform* readers by replacing Christianity with a more rational, egoistic outlook. On Johnston's reading, the skeptical critique of religion in the second half of *Leviathan* is central to this objective; for it is here that the task of subtly eroding the Christian worldview is undertaken. Accordingly, Hobbes' debunking of miracles is part of a larger effort to subvert "the entire mental outlook" on which Christian traditions and institutions rest (Johnston 1986, 158).

Against this view, it has been argued by J. G. A. Pocock (1971) and, more recently, Aloysius Martinich (1992) that Hobbes was a more traditional theist than he is commonly supposed to have been. Martinich claims that while his doctrines may have been unconventional or *nonstandard* for the time, Hobbes' views were far from *unorthodox*. Luther, Calvin, and other Reformation luminaries had expressed similar doubts about the reported occurrence of miracles. Yet this does not lead critics (nowadays, at least) to question the genuineness of their theism. "Hobbes has been the victim of a double standard," Martinich complains (ibid., 4). He goes on to argue that the second half of *Leviathan* should be read as a sincere (if flawed) attempt to reconcile Christianity with the science of Copernicus and Galileo.

I do not find this a very persuasive interpretation. Hobbes' *doctrine* may not have been unorthodox, but his *rhetoric* surely was. He peppered his remarks on prophecy, inspiration, and other elements of popular religion with scathing wit and biting satire, none of which comes through in Martinich's account of Hobbes' theism.[14] I agree with Johnston: to understand the second half of *Leviathan*, it is necessary to probe beneath the surface of its language: I there discover not rational egoism but skeptical moralism. As in *Behemoth*, Hobbes seems to have been less interested in *transforming* readers, or *reconstituting* them, than in encouraging them to hold their views (whatever they happen to be) more circumspectly and to express them more discreetly. That is, he was less interested in *what* subjects believed than in *how* they held and expressed their beliefs. He was trying to instill the virtue of judgment (or discretion) he had described in *Leviathan*, chapter 8.

"A private man has alwaies the liberty, (because thought is free,) to beleeve, or not beleeve in his heart, those acts that have been given out for Miracles" (1968, 478). I take Hobbes at his word here. His concern was not with the content of belief (rational egoism vs. religion) but the manner of belief and its expression. As subjects, Hobbes said, "we are not every one, to make our own private Reason, or Conscience, but the Publique Reason, that is, the reason of Gods Supreme Lieutenant, Judge" (ibid. 477). Again, however, as in *Behemoth*, he did not present this conclusion as the result of a logical, deductive argument. It is, rather, something that should arise naturally within us, the result of our skeptical disposition. The point of his discussion of miracles (which immediately precedes this conclusion) is to nurture this disposition. We are not told that witnesses to a miracle are frauds or that only fools believe in miracles. Presumably, Hobbes thought so; and he wanted us to think so, too. But his purpose was not to plant the seeds of doubt and thereby overturn religion. It was, rather, to instill in us the capacity for skeptical, self-critical detachment. Thus, whatever our private judgment (Catholic, Protestant, or egoistic), we will defer willingly and naturally to the dictate of "Publique Reason."

It will perhaps be objected that while we may need intellectual virtue (or skeptical moral judgment) in order to create a Hobbesian commonwealth, there is no place within such a commonwealth for the kind of virtuous intellect I have been touting. As subjects, Hobbes said, "wee are bidden to captivate our understanding" to that of the sovereign; to "forbear contradiction," and to "speak as (by lawfull Authority) we are commanded" (1968, 410). Independent judgment has, in effect, been eradicated. What need have we, then, of skeptical judgment or intellectual virtue?

The problem with this objection is that it overlooks the complexity that I have been discussing.[15] The subjects of Hobbes' commonwealth are divided selves, pulled in one direction by conscience and another by law. The tension within their psyches does not disappear when they become subjects. Indeed, one might say that the practical, polemical point of Hobbes' writing on religion was to bring this tension to light, to make its presence felt. Refusing to acknowledge the "Publique Reason" of "Gods Supreme Lieutenant," his fellow citizens "clamor and demand right Reason for judge; yet seek no more, but that things should be determined, by no other mens reason but their own" (1968, 111). This, of course, is intolerable; but the people of England, victims of moral illusion, could not see what is intolerable about it. In the name of justice, these people attacked the very institution that exists to make a just life possible. Like the "emotivists" described in Alasdair MacIntyre's (1984) *After Virtue*, they refused (on moral grounds) to render the act of submission that moral coherence requires. As a skeptical moralist, Hobbes' aim was to disclose to them the incoherence of their position by demonstrating the need to think of themselves as divided selves. We must respect law, or "Publique Reason," as well as conscience; otherwise, the word 'conscience' can signify no more than those among our opinions that we cling to stubbornly and self-righteously.

Interpreted in this way, it makes sense to situate Hobbes in a tradition with Montaigne and the other Renaissance skeptics, not, as Richard Tuck argues, because this tradition seeks to annul independent judgment but because it refines and civilizes it. As I have tried to suggest, Hobbes regarded the concept of divided selfhood in the same way that Montaigne did. For both, it represents a kind of moral achievement, a deeper self-awareness, according to which self-critical detachment and respect for law (Hobbes) or custom (Montaigne) is as important as the willingness to stand by one's convictions resolutely. Both, moreover, sought to instill the skeptical habits of mind that enable us to manage the tension between law and conscience that this achievement brings with it. For these reasons, I conclude that like Montaigne, Hobbes was not only a skeptic but a skeptical moralist.

CONCLUSION

At one time, people constructed their dwellings in total ignorance of the rational principles upon which "the art of well building" later came to be founded. But "Time, and Industry," have since remedied this defect, and our houses are more secure and longlasting as a result. A similar development, Hobbes believed, may be expected in the art of well governing: "So, long time after men have begun to constitute Common-wealths, imperfect, and apt to relapse into disorder, there may, Principles of Reason be found out, by industrious meditation, to make their constitution (excepting by externall violence) everlasting" (1968, 378). An optimistic scenario, indeed, and hardly what one would expect of a doubting, hesitant skeptic. So, a critic might contend, skeptical moralism may be a plausible interpretation of that part of *Leviathan* which has to do with religion. But is it not misleading to suggest that *Leviathan* as such is the work of a skeptical moralist?

I would say that it is not; and, although space does not permit a full-fledged defense of this claim, I want to conclude by touching briefly on the connection between science and skeptical moralism, both in Hobbes' political theory and more generally. There is no doubt that according to Hobbes' science of politics, it is the desire to preserve ourselves from harm that leads us to surrender our natural right of self-preservation in exchange for the protection afforded by a commonwealth. Reasoning scientifically, each can see that to struggle alone for self-preservation is a futile undertaking and that the end we all seek (i.e., our safety and comfort) is more likely to be achieved by compromise than by combat. To bring us to that realization is, surely, one of the purposes of Hobbes' science of politics.

But it is not the only purpose. There is, I suspect, more to Hobbes' science than the attempt to justify political obligation. Skeptical moralism is important because it can help us to see what is missing from this account. When people come together to form a commonwealth, they do not move from one amoral condition (the state of nature) to another but from a condition of moral blindness or illusion to one of moral enlightenment; and scientific reason plays an important part in this development. People in the state of nature speak a moral language, but they do so naively. Like the Presbyterian preachers, who failed to take seriously the possibility that sanctity might be feigned, people in the state of nature fail to realize that "what is called right, what good, what virtue," and so on (1840, 4:225) is in part a reflection of the speaker's own, narrow prejudices. It is only by attaining the detached standpoint afforded by civil law—the "common standard of virtues and vices" provided in a commonwealth (1978, 69)—that we acquire the competence to judge self-critically, hence to become truly moral agents. Insofar as scientific reason helps us to attain this standpoint, it has an important role to play in our moral development.

Science and skeptical moralism complement one another in Hobbes' theory. Whether he was writing as a scientist (in the first half of *Leviathan*) or a skeptical moralist (in the second half), Hobbes' intent seems to have been to make his readers more aware of the ways in which moral language is susceptible to abuse—not for the purpose of subverting it but to deepen and refine the subject's moral sensibility.

James Farr has argued that at the time of its origin (during the Scottish Enlightenment), the science of politics was understood to be "an instrument of political education and criticism in the interests of moderation." That is, at its inception the science of politics was *both* a method for studying the world *and* a means of instilling a political ethic, in which a premium was placed on the competence for detached, self-critical reasoning (1988, 57). I am suggesting that we can read Hobbes' "Morall and Civill Science" (1968, 239) in much the same way—as a method for studying politics and as an attempt to instill a genuine civic morality. Like Farr's Scots (notably David Hume, Adam Smith, and Adam Ferguson), Thomas Hobbes regarded science as a tool for the development of moral character. We may not approve of the kind of moral character Hobbes sought to develop; but we should take seriously his claim to have been a *moral* scientist, because we, as citizens, wrestle with some of the same difficulties and because the self-critical ideal for which the luminaries in our discipline once stood may yet be worth recovering.

Notes

For their helpful comments and insightful criticism, I would like to thank Donald Hanson, my colleagues Russ Hanson and Jeff Isaac, and my teachers, Terry Ball, Mary Dietz, and (especially) Jim Farr.

1. Montaigne distinguished among several types of classical skeptics. In the passage from which I have quoted, he was referring to Pyrrhonian skeptics. Unlike Academic skeptics (who denied that we can have knowledge of the world as it really is), Pyrrhonists denied that we can even know so much as to say (with the Academics) that we cannot know the world as it really is. Pyrrhonists regarded Academic skeptics as negative dogmatists. For an illuminating discussion of ancient skepticism and its varieties, see Annas and Barnes 1985; Burnyeat 1983.

2. One can find similar accounts of the intellectual virtue of judgment in *De homine*, chap. 13 (1978, 63–70) and *Elements of Law*, pt. 1, chap. 10 (1840, 4:54–59). It should be noted that the account in *De homine* is more relativist in tone than either of the other two. Here Hobbes did not characterize judgment either as an intellectual virtue or as a "virtue of the mind" (1840, 4:55–56) but simply as a disposition or competence. One who has this competence is able to do things that those without it can only do more clumsily (if at all).

3. The corresponding chapters of *Elements of Law* and *Leviathan* in which similar renditions of the law of nature can be found are *Elements of Law* pt. 1, chaps. 16–17 (1840, 4:95–111); and *Leviathan*, chap. 15 (1968, 201–17). There are some notable differences among the three versions. For example, only in *De cive* (chap. 3, sec. 30) does the portrait of the "just man" appear.

4. The connection between virtue and peace is considerably more ambiguous in *Elements of Law* than in the other two versions: "The sum of virtue is to be sociable with them that will be sociable, and formidable to them that will not. And the same is the sum of the law of nature" (110–11). In *Leviathan* and *De cive*, to be formidable to one's foes is not described as a "virtue."

5. R. E. Ewin 1991 and George Shelton 1992 have also sought to illuminate the place that virtue occupies in Hobbes' theory. Yet as informative as they are, they do not explore the relevance of Hobbes' skepticism to his thinking about civic virtue.

6. For the view that one can find in Hobbes a limited doctrine of toleration, see Ryan 1988; Tuck 1990. While I find the tolerationist reading intriguing, my interpretation moves in a somewhat different direction. I think Hobbes was less interested in instilling an attitude of toleration than in confronting his readers with the inner conflict that, in his view, necessarily bedevils moral agency. The experience of being deeply at odds with oneself may make one a more tolerant person; but (as I hope my argument will make clear) toleration does not exhaust the significance of the skeptical moralist's ethic.

7. Richard Tuck reports that *Of Wisdom* went through 12 French and 8 English editions from 1601 (the date of its original publication) until 1670 (1993, 83).

8. A "man of understanding," Montaigne said, "should withdraw his soul within, out of the crowd, and keep it in freedom and power to judge things freely; but as for externals, he should wholly follow the accepted fashions and forms. Society in general can do without our thoughts; but the rest—our actions, our work, our fortunes, and our very life—we must lend and abandon to its service and to the common opinions" (1957, 86).

9. "To keep oneself wavering and half-and-half, to keep one's allegiance motionless and without inclination in one's country's troubles and in civil dissensions, I consider neither handsome nor honorable" (1957, 601). Montaigne spent nearly 18 years (approximately one-third of his life) in public service, both in the Parlement of Bordeaux and as the city's mayor (Frame 1965). Still, he looked upon public life with deep suspicion: "If people have sometimes pushed me into the management of other men's affairs, I have promised to take them in hand, not in lungs and liver; . . . to be concerned over them, yes; to be impassioned over them, never" (1957, 767).

10. Space does not permit more than a brief mention of one other figure in the tradition of Renaissance skepticism: Justus Lipsius. While Lipsius is usually read as a neo-Stoic, rather than a skeptic (see Tuck 1993), the tension between conscience and custom is a theme in his writing as well. Like Montaigne, Lipsius was deeply suspicious of those who regard politics as a forum for expressing moral views. But one need not conclude from this (as Tuck has) that he sought wisdom in the renunciation of moral conviction. At least, in his dialogue *Two Bookes Of Constancy*, Lipsius seems to have been more interested in the refinement of moral judgment than its annulment.

11. Hobbes was quoting here from an Anglican document, *The Whole Duty of Man Laid Down in a Plain and Familiar Way*. Judging by the contents of this document, which was the work of "some divine of good reputation and learning, of the late King's party" (1990, 47), the Presbyterian ministers would, he thought, be exonerated. His point was to demonstrate the futility of relying on traditional Christian thought to expose the misconduct of rebellious preachers.

12. Hobbes made one of the interlocutors in *Behemoth* say, "Hypocrisy hath indeed this great prerogative above other sins, that it cannot be accused" (1990, 48).

13. In reading *Behemoth* this way—as a polemical work, with moral education as its aim—I am following the lead of Dietz 1990 and Farr 1990. As I have already mentioned, Dietz argues that Hobbes sought to "reconstitute" subjects as citizens and that his political writings—*Leviathan*, as well as *Behemoth*—were written with this end in mind. Similarly, Farr maintains that Hobbes (in parts 3 and 4 of *Leviathan*) was appealing "over the head of the sovereign" to his readers, in an effort to "reconstitute" them as obedient subjects (1990, 185, 187).

14. Having said this, I should hasten to add that the conception of moral agency I am attributing to Hobbes, involving as it does the idea of selves carrying on an internal struggle, has deep roots in Protestant theology. "It is a most difficult and arduous achievement to renounce ourselves and lay aside our natural disposition," John Calvin observed. "And, indeed, as we are naturally averse to God, unless self-denial precede, we shall never tend to that which is right"

(1983, 1:515). But for Hobbes, the point of struggling with oneself was not to advance a particular religious orthodoxy. It was, rather, to instill a more critical attitude toward orthodoxy as such. Thus, it seems to me, Hobbes was less a Protestant than a skeptical moralist.

15. It also overlooks the passage immediately following Hobbes' remark about "captivating" the understanding. "But by the Captivity of our Understanding," Hobbes went on to say, "is not meant a Submission of the Intellectuall faculty, to the Opinion of any other man; but of the Will to Obedience, where obedience is due" (1968, 410). Responding to Bishop Bramhall's accusation, that "He subjecteth all prophetical revelations from God, to the sole pleasure and censure of the sovereign prince," Hobbes replied: "I never said that princes can make doctrines or prophecies true or false; but I say every sovereign prince has a right to prohibit the public teaching of them, whether false or true" (1840, 4:325, 329).

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RESEARCH NOTES

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SEPARATING PARTISANSHIP FROM PARTY IN JUDICIAL RESEARCH: REAPPORTIONMENT IN THE U.S. DISTRICT COURTS

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Previous judicial decision-making studies have used party labels as surrogates for ideological orientation. In this study of state-level reapportionment cases before the U.S. district courts, I investigate the partisan reference-group identification of judges as a nonlegal influence on decisions. The partisan identification of judges is juxtaposed with the political orientations of the legislatures passing reapportionment plans. The results suggest that partisan effects are included in the pattern of decisions in cases related to the state-level district plans. While judges tend to vote against plans submitted to them from legislatures where one political party has numerical control of the reapportionment process, they vote against plans presented from legislatures of their own party at a much lower rate than in cases where the party opposite their own controls the district drawing process. The logit model correctly predicts 89.3% of the cases and produces a 60.3% reduction of error.

Political party labels have had a prominent position in American judicial research for many years, where they have substituted for the ideological orientations of judges that are not easily found any other way. Studies using party labels have operated as though liberal decisions are expected from Democratic judges and conservative decisions from Republican judges and that like-minded individuals will belong to the same political party (Tate and Handberg 1991). Party designation as a representation of ideological orientation has been found to explain decision making in civil liberties, economics, and other legal areas at various levels of American courts (Carp and Rowland 1983; Goldman 1966, 1975; Nagel 1961; Songer and Davis 1990; Tate 1981; Tate and Handberg 1991). However, the use of party labels confuses or blurs the distinction between the roles of partisanship and ideology in judicial decisions, leading to the impression that "Democrat" and "Republican" explain votes, not "liberal" and "conservative."

Partisanship and ideology represent two separate phenomena, and using party labels solely to represent ideology misses the possible relationship between an affective, or psychological reference-group identification held by a judge and the decisions the judge makes. Reference groups are often discussed in relation to elections, where the electoral cue for a voter is a political party or other group with which the person identifies (Berelson, Lazarsfeld, and McPhee 1954; Campbell et al. 1960). More specifically, political group adherents want their group or party to succeed both in elections and in policy creation: "The prime group goal is political victory" (Campbell et al. 1960, 327). Judges should not be immune to such feelings. If a person's party provides this cue-giving function and a sense of belonging, then models employing party labels as an indication of ideology, without a partisan component, may have led to misspecified models. If judges are attracted to parties in the same way that others are, then there may be a partisan, nonideological component to their decisions.

A check for this expected partisan influence requires some way to connect the partisan identification of judges with the partisan nature of cases before them. Not all case types are appropriate for a check of partisanship, but reapportionment controversies in the U.S. district courts offer a logical way to test this proposition. First, the cases have decidedly partisan implications. The future of a party's legislative strength is often assumed to be related to the outcome of the district drawing process. Second, these plans are likely to be created along partisan lines in state legislatures as a means to further the group success.[1] I shall use a logit analysis of state-level reapportionment cases from 1964 to 1983 to examine the expected reference-group impact on decisions in district courts.

REAPPORTIONMENT

The history of federal court intervention in reapportionment plans of the states is relatively short. Not until 1962, in *Baker* v. *Carr*, did the Court recognize the jurisdiction of reapportionment plans under the equal protection clause of the Fourteenth Amendment, acknowledging that voters had standing under the Constitution. Two years later in *Reynolds* v. *Sims* (1964), the Court gave initial guidance on what formed an acceptable reapportionment plan.

For state-level plans, the major outline for acceptability included both population limits and boundary-line patterns related to some "community of interest." The "one person, one vote" standard from *Gray* v. *Sanders*, applied in *Reynolds*, concludes that "as a basic constitutional standard, the Equal Protection Clause requires the seats in both houses of a bicameral state legislature must be apportioned on a population basis" (1964, 576; see *Gray* 1963, 381).[2] The exceptions were "legitimate considerations incident to the effectuation of a rational state policy" (*Reynolds* 1964, 579). This allowed unequal districts when boundary lines were drawn for a compelling reason of policy.

In 1967 the Court set forth, more explicitly, the possible considerations that would make a plan with large population deviations acceptable. A state plan was struck down in *Swann* v. *Adams* (1967), which had over 20% population variances in both houses, based on the state's lack of justification for drawing unequal districts. The Court said that possible justifications would be "integrity of political subdivisions, the maintenance of compactness and contiguity in legislative districts, or the recognition of natural or historical boundary lines" (p. 444).

While the Court refrained in early decisions from making any specific judgment as to the allowable deviation from population equality, cases were cited by district courts as limiting the variance. In *Lucas* v. *Colorado General Assembly* (1964), a decision announced on the same day as *Reynolds*, the Court held a population variance ratio of 1.7 to 1—computed by dividing the number of people in the most populous district by the number of people in the district with the lowest population—to be marginally acceptable. This provided a guideline to lower courts for judgments regarding population deviations in state reapportionment plans.

The acceptable deviation limit guideline for the judges was lowered in a district court case in 1965—*Toombs* v. *Fortson*—where a 15% single-district deviation from population parity, or a 30% upper-to-lower-district population total deviation, was suggested as a limit conforming to a bill then before Congress and meeting the suggestions of a committee of the American Political Science Association.[3] Beginning about 1973, the Supreme Court announced a series of decisions that labeled plans with population deviations below 10% as de minimis, or trivial, and set a new total deviation limit at 16.4%.[4]

The analysis outlines the influences expected to describe judicial decisions in reapportionment cases in the district courts. The primary question is, Is there a partisan, reference-group attachment that can help to explain the decisions of judges in these cases?

DATA

For the present study, state-level district plans, unlike those related to congressional district plans,[5] offer the opportunity for greater population deviations and therefore greater opportunity for attitudinal influences to be involved in decisions. The cases for this study are coded from the *Federal Supplement* from 1964 to 1983.

The decisions published in this supplement are thought to represent about 5% of those that come before the district courts (Carp and Rowland 1983, 16). There is no way of knowing what percentage of reapportionment cases are represented, and some criticism has recently been leveled against the use of published district court cases (Olson 1992). Much of this criticism is not applicable here. First, this study is not concerned with cases that do not reach the decision stage, since it is the direction of final decisions that is of interest. This one point minimizes a great deal of the concern for using published district court cases. The remainder of the concern has to do with the writing and publication of final decisions. Whether the cases are published is supposedly related to their importance and interest to the legal profession. Since reapportionment based on population constraints has been of interest to many legal practitioners and continued to be unsettled until the early 1980s, most of the cases should have been published. Due to their important nature, I assume that most of the cases are included in the *Supplement*.

An attempt was made to find all reapportionment cases in the district courts. The simplest place to start was a list of cases heard by the Supreme Court, collected in the *United States Supreme Court Database* (Spaeth 1991). I examined the notations in the *Supreme Court Reporter*, and took the citation from the district court. Not all of the reapportionment controversies go to the Supreme Court, so a Westlaw search was created from the keys related to the material found in the Spaeth data set. Other citations were obtained from various district court reapportionment decisions.

The search came up with 44 usable occurrences of reapportionment controversies. Some cases covered a single district or county and were excluded because they did not lend themselves to the primary question of statewide partisan plans being drawn by state legislatures.[6] Other reports represented continuances, generally including preliminary opinions, for which a later report gave a final decision.[7] These continuances were excluded to avoid duplications. When multiple disputes were combined into one case, only a single finding (the one that most closely followed the research design) was coded. Partial dissents were coded based on the portion of the decision related to the influences under investigation.

Statewide reapportionment cases in the district courts are heard by a three-judge panel.[8] Each judge's opinion was coded, thus increasing the number of observations by a factor of three.[9] Further, many decisions dealt with questions arising from both lower- and upper-chamber state district plans. Controversies with both sides of the legislature at issue were divided, because they had differing variances and boundary considerations, and the plan for each house was treated separately. This brought the number of case–judge observations to 196, representing 87 judges and cases from 28 states.[10] The period begins with the date of the *Reynolds* v. *Sims* (1964) decision, the case requiring both houses of the legislature to be properly apportioned. The last observations were from 1983, because aspects of reapportionment concerning population equality were well established by then. Reapportionment disputes following this date deal with questions of census enumeration and political gerrymandering,[11] among others, and do not follow the model where the concern is population deviation. Also, majority-minority district cases follow the changes to the Voter Rights Act in 1982, after the period studied, and deal with a question not central to this study.

MODEL

A recent study of Supreme Court decision making argues that model analysis, to be properly specified, requires both legal and nonlegal controls (George and Epstein 1992). In the present model, controls for both case facts and attitudinal attributes are included. The attitudinal influences here are those related to judge *characteristics*, which include partisan reference-group attachments; *ideology*, as represented by the party of the judge; and differentiation by *region*. The legal controls are the facts related to reapportionment cases: the deviation from population parity, boundary considerations, and a control for time periods related to the settling of precedent.

Dependent Variable

The dependent variable is the vote by each judge in each case. All votes are coded by direction—either for the plaintiff, against the plan (coded 1) or for the defendant, upholding the plan (coded 0).[12] The dependent variable in this study was dichotomous, and the analysis employed a logit regression model. Logit provides the maximum likelihood estimates (MLEs) from which the impact of independent variables on the dependent variable can be calculated. Also, standard errors are provided and, since the estimates are asymptotically distributed, statistical significance can be calculated and interpreted, much as when using ordinary least squares regression.

Attitudinal Variables

The three attitudinal variables were, again, partisanship, ideology, and region.

Partisanship. For partisanship influences, there are two expectations. First, a judge would know which party was in control of the state government and even who drew up the reapportionment plan at issue. Also, a judge would be inclined toward acceptance of that plan if there were a congruence of parties with the judge, against acceptance of the plan if it were drawn by a party different from that of the judge.

The party of each judge and the party of the legislature that drew up the reapportionment plan determined reference group variables. First, the party of the judge was found in published sources, such as *Who's Who in American Law*.[13] Second, the state legislative party levels were found in various volumes of *The Statistical Abstract of the United States* for the year the plan was passed. Party control for drawing the plan was ascribed through a threshold of partisan legislative strength. If both houses of the legislature were controlled by a majority of any party and the governor was of that party—as a control for a veto power—then the plan was attributed to them.[14] If a party held two-thirds or more of the seats in a legislature, then the governor was left out on the assumption that a veto could be overridden.[15] Plans not identified as partisan in this manner are nonattributed.

Next, variables were coded representing the relationship between the majority party in the legislature of each state, which drew up a contested plan, and the partisan identification of the judges who sat in each case. If the party of the judge and legislature were the same, then the variable noted a same-party relationship. This was regardless of the party involved, since there was no expectation that the impact varied by party. Should the party of the judge have differed from that of the attributed legislature involved in making up the plan, the variable noted a different-party relationship. When the plan was not attributable to any party, due to a lack of party control in the legislature or an opposing party governor, then the decision outcome was expected to be a result of the legal and ideological influences alone. That is, with no party in the legislature able to impose its view of a plan on the other, the plan should have been nonpartisan and would have presented no reference group identification for the judge.

The nonattributable plans created a zero coding on both party relation variables and were considered neutral or pivotal in the model. The same-party influences should have negative magnitudes, and the different-party influences should have positive magnitudes, as compared to the neutral cases. SAME PARTY and DIFFERENT PARTY are dummy variables for the judge and legislature party criteria: 0 for an observation that did not fit both criteria, and 1 for an observation that fit the party of the judge and the party of the legislature for each variable. (The observations with plans that were not attributable made up a separate reference category.)[16]

Ideology. Ideology should be a factor in the decisions of reapportionment cases. For instance, a liberal judge, seeing a plan as a breach of civil liberties effecting underdogs, may have been more inclined than a conservative judge to vote against a malapportioned plan. Reapportionment reform—seeking to equalize the weight given to each voter—is liberal when the alternative is inequality. In addition, conservative judges may have been less inclined to overturn a legislature's efforts either from the standpoint of meddling in the affairs of another branch of government or from a states' rights perspective, indicating a judicial conservatism.

During the period under review, intervention decisions were one-sided. To intervene was to require greater equality than the legislature provided, not less equality. This is considered a liberal decision. Care should be taken here not to equate the disposition by a judge to strike down a plan as a group attachment. Any consideration about the liberal or partisan nature of the constituency affected by a plan was assumed to be controlled for in the nature of the party that created the plans in the legislature. That is, it was assumed that a Democratic legislature attempted to draw plans for the benefit of their constituents, as would a Republican legislature with respect to their constituents. It was the judges' ideology I controlled for here (e.g., the likelihood of meddling in a sovereign state's policies), not who was advantaged by the plan. Ascribing direct judicial involvement to constituent advantage was not the intent.

Finding a measure for the ideology of any judge, however, can be difficult. That is why party labels have been so popular. Although recent judicial research uses the party of the appointing president and individual administrations to represent ideology (Songer and Haire 1992; Tate 1981; Tate and Handberg 1991), in this data set there were too few judges appointed by each president to represent individual administrations, so only a party distinction was included. The party of the judge was used to assess any ideological coherence along party lines. The expectation was that Democratic and Republican judges had fundamentally different ideological attitudes resulting in different decisions. PARTY ID is a dummy variable identifying the party of the judge (a 0 was coded for a Republican judge, and a 1 for those judges identifying with the Democratic party).[17]

Regional Controls. As with previous research on federal and state courts, a regional variable was employed as a control for the difference in impact between southern and nonsouthern judges. First, judges were likely to be socialized into the ideological culture of the regions in which they were raised. Second, the judges are likely to have been raised in the region for which the appointment came. For the few judges who moved to the region as adults, I relied on their conforming to their surroundings, or taking on the characteristics of those with whom they come in contact. The decisions of southern judges as compared to nonsouthern judges are understood to reflect a more conservative position on civil liberties, and economics decisions (Carp and Rowland 1983; Gryski and Main 1986; Songer and Davis 1990; Tate and Handberg 1991). As reapportionment is a civil liberties issue, the expectation was that southern judges would be less likely to strike down a poorly drawn reapportionment plan, remembering that a vote to strike a plan meant requiring population equality. Conservative attitudes in reapportionment should have been consistent with a southern states' rights position.

Any concern for southern judges attempting to control against the redistricting of minority or rural districts to accomplish fair representation was minimal. Only a plan that was malapportioned on a population basis was heard by the courts. Any plans contested solely on boundary considerations did not appear in the data set. Since the redistricting of minority areas would have to go toward population equality, even if a judge were opposed to minority representation, there was no chance to reverse a well-apportioned plan. Only less-than-perfect plans could be voted against, which is still a liberal position in this model.

Cases arose from 11 of the 15 southern and border states.[18] The SOUTHERN CASES variable was coded according to the location of the district court (0 is coded for non-South cases, and a 1 for those that come from the South). For those few judges who sat on a case from a different region (e.g., Tennessee belongs to a circuit that includes both northern and border states), the district from which the judge came was used to determine the region. Slightly more than one-third of the cases and observations were from the South.

Case Facts

The case facts involve, again, population variance, boundary considerations, and the settling of precedent.

Population Variance. Segal (1984, 1985) studied the impact of case facts relative to precedent, using the particulars in search-and-seizure cases to represent the influence of legal factors on decision outcomes. The first case fact in the present model was the deviation from population equality. A determination of whether the plan met the acceptable ratio for the period was included. A simple dichotomous variable was coded for whether the plan was over or under the *Lucas* guideline for cases up to 1965, the (*Toombs* v.) *Fortson* guideline for cases from 1965 to 1973, or the *Mahan* guideline for cases after 1973. The expectation for this variable was that the probability of a vote against a plan would increase if the deviation was above the outlined limit. POPULATION is a dichotomous variable indicating the adherence of state plans to the deviation limits in place at the time the case was decided (0 for under, and 1 for over).

Boundary Considerations. The second case-fact variable in this model was a control for the appropriateness of the boundaries in reapportionment plans. Unfortunately for this study, and unlike Segal's study, the district courts were the first steps in the litigation of reapportionment cases. Where Segal could use case facts established in lower courts for his analysis of the Supreme Court, the facts presented in the case must be used here to determine the various influences. For the population deviation, the concern that a decision was due to cooked numbers was not great, since the deviation levels were, for the most part, objective measures. For boundary conditions, though, the test of acceptability was the community of interest and a compelling state policy in drawing lines along a certain geographic or municipal boundary. This determination was fairly subjective, and boundary-line leeway gave judges some maneuvering room to justify noncompliance with the established population ratio precedent. The boundary conditions were therefore suspect.[19]

Along the reasoning that litigants in the case reached a threshold in which a pure population argument was not enough, I included in the model boundary contentions in the cases, rather than any determination by the court of boundary appropriateness. Given the usefulness of boundary leeway, a party to the case might have included the argument to allow a sympathetic judge a way to justify a decision. The inclusion of the boundary question in a case was included as a control for this discretionary ability. Although this control is not as precise as could be hoped for, controlling for an argument in this way has found some success.[20] Since the boundary question can justify a decision for or against the plan, no expectation is proposed as to the direction of the impact. BOUNDARY represents the inclusion of a boundary-line question (a 0 was coded for cases that did not contest the boundary lines, and a 1 was coded for those where such a question was included).

Settling Precedent. Finally, a time-period variable was included to control for possible reactions due to the settling of precedent. Before *Swann* in 1967, the boundary-line justification for deviating plans was not well outlined. The settling of precedent should have had the effect of limiting the discretion of judges, thus resulting in a reversal of whatever direction that discretion had been taking, either for or against plans. A dichotomous variable was included here to control for the cases that occurred before and after the *Swann* decision.[21] POST-SWANN is a dichotomous variable related to the 1967 *Swann* v. *Adams* case (0 for prior, and 1 for after).

TABLE 1

Maximum Likelihood Estimates Predicting Judges' Votes to Overturn Reapportionment Plans in U.S. District Courts, 1964–83

VARIABLE	MAXIMUM LIKELIHOOD ESTIMATE	STANDARD ERROR	IMPACT[a]
Intercept	−5.24	1.24	—
Same party[b]	2.11	1.14	.25*
Different party[c]	5.83	1.30	.55***
Party ID[d]	2.31	.72	.36**
Southern cases[e]	−1.12	.53	−.17*
Population[f]	2.70	.68	.32***
Boundary[g]	5.62	1.16	.71***
Post-Swann[h]	−1.15	.60	−.17*

Note: Percentage correctly predicted = 89.3 (false pos. = 16.4%, false neg. = 7.3%); reduction of error = 60.3%, adj. $R^2 = .50$; $-2LLR = 255.5$; model $\chi^2 = 101.4$, df = 7, $p < .0001$; N = 196, mean of dep. var. = .64.

[a] Impact is calculated by formula for each independent variable by holding all other variables to their mean across the upper and lower limits of the variable in question (see Segal and Spaeth 1993 for further discussion).

[b] 1 when the judge's party and the legislative majority are the same, 0 otherwise. The reference points, against which these party relationship variables are compared, are the nonattributable cases with no legislative majority (i.e., a 0 coding for both the same- and different-party variables).

[c] 1 when the judge's party and legislative majority are different, 0 otherwise.

[d] 1 for a Democratic judge, 0 for a Republican.

[e] 1 when the case is from the South, 0 when from the non-South.

[f] 1 when the population inequality is greater than the accepted limit, 0 when below the accepted limit.

[g] 1 when a boundary contention is included in the case, 0 if not.

[h] 1 if the case is after the *Swann* decision in 1967, 0 if before.

* $p \leq .05$.

** $p \leq .005$.

*** $p \leq .0005$.

RESULTS

The logit model provides a good indication of the influences on district court reapportionment decisions. The model attains a .0001 level of significance, correctly predicts 89.3% of the cases, and results in a 60.3% reduction of error.[22] In addition, all of the variables attain statistical significance. The impact of each exogenous variable is dependent on the value of all of the other variables. Thus, in this study, the impact of each significant independent variable is calculated by formula in which all other variables are set at their means—indicating an even chance of their occurrence.[23] For the dichotomous dummy variables, an occurrence corresponds to a change in the probability of finding a plan unacceptable. Table 1 shows the MLE coefficients with their standard errors and impacts.

The central concern of this study is whether party labels reflect a different influence on decisions from partisan reference-group attachments. The expectation was that where there is a same-party congruence between the judge and the legislature drawing up the plan, the decision would be in a negative direction as compared to the nonattributed, reference category cases. This does not occur. Both partisan variables are positive in direction, suggesting a greater probability of voting against any partisan plan over a nonpartisan plan. For the different-party decisions, the result is consistent with expectations about cross-group partisanship. A strong partisan influence is evident, which indicates that judges take either conscious or unconscious note of the party that draws up the plans in state legislatures and hold opposite party plans to a strict standard. Yet the same-party plans are also held to a strict standard as compared to nonpartisan plans.

Why would partisan judges vote against plans created by their own party? Of note in the results is the DIFFERENT PARTY impact, which is twice that of the same-party variable and signifies a relative difference in partisan influence. The probability of a reapportionment plan being voted against when the judge holds an opposite-party identification (DIFFERENT PARTY) to that of the legislature drawing up the plan is .55 greater than when the legislature is nonpartisan; the SAME PARTY probability is .25 greater than for the neutral legislature plans. This may indicate a partisan component, as expected, where the affective or psychological attachment leads judges to decide cases more severely for the opposite party—and also that the judge may be sensitized to a favorable same-party bias for which a conscious correction is made in the decisions, leading to a stricter standard than would normally appear.

One concern may be that partisan plans are likely to be more egregiously malapportioned in the first place. That may be. However, that does not explain why same-party cases, among all partisan plans, are less likely to be voted against. Legislatures would have to expect their plans to go to court and make some calculation as to the judges who would be hearing the case for the population inequality to be related to partisan impact. For whatever reason, in the end, partisan-created plans are more likely to be struck down than are nonpartisan plans, and partisanship does influence decisions.

The PARTY ID variable, representing the party identification label of each judge, provides evidence of an ideological divide between liberals and conservatives, as is found in other research. Democratic judges are more likely to vote against a reapportionment plan than are Republican judges (.36). This is consistent with expectations that reapportionment is a civil liberties issue where conservatives are more sparing in their antiplan decisions than are their liberal counterparts. The results support the expectation that there are influences from both partisanship and ideology related to party identification: party identification as a measure of ideology still produces significant results after including reference-group controls in the model.

A control for region extends the support for ideological distinctiveness. The expectation was that there would be a differentiation in the ideological direction of these cases by region of the country. The South, with a more conservative, states-rights tradition among its people, and the possibility that reapportionment reform might be equated with court-imposed desegregation, should be less likely to see plans stricken. The expectation is met: the non-South, as the reference category, is the region with the greatest likelihood of a vote against a plan; the South, as a more conservative region, is less hospitable to intervention (−.17).

Legal controls for the case facts predict a large portion of the decisions also. The variable included to control for the boundary line questions (BOUNDARY) has the largest impact on the probability of a vote against the plan (.71). This suggests a strong relationship between the inclusion of the boundary question and a decision against the plan. If a plan has a minimal amount of deviation from population, there should be no need to include the boundary justification as a support for the defense, and a judge can hardly say that a boundary is bad if there is no population deviation question in the first place. Therefore, only if there is a large deviation will a question arise over the boundary. This is consistent with the expectation that a threshold is reached in which the degree of deviation tends to bring about a boundary contention.

Of the other two legal variables, the population variance takes second place. With POPULATION, which represents the dichotomous population variance distinction, the impact is an increased .32 probability of a vote against a plan, or less than one-half the boundary variable impact. Population deviation seems to be less of a concern to judges than the justification for unequal districts and is in line with the published Supreme Court cases allowing boundary-line discretion. The boundary variable controls for the extremity of population disparity that the dichotomous population variance variable does not.

The impact for the year variable (POST-SWANN), indicating the effect of precedent on the decisions, is in the negative direction. After *Swann* v. *Adams* in 1967, the probability that the reapportionment cases would be voted against was lower than before (−.17). One reason for this could be that judges had previously been holding cases to a stricter standard because population parity was considered paramount in the plans. With the *Swann* decision, some greater flexibility was created, making population equality of districts just one consideration.

In the extremes, the cases from the non-South before 1967 with a different-party relationship were the most likely to be struck down. Conversely, post-1967 southern cases with nonpartisan legislative majorities had the least likelihood of being voted against.

CONCLUSION

At least in some judicial decisions, ideology and attraction to a party have separate and measurable influences on the outcome of cases. Certainly, partisanship and ideology are closely related in a political system where policy is organized around the contests between supposedly ideological combatants distinguished by party labels. Ideology represents a person's view of the world and on a court it represents a predisposition toward who should be advantaged—the underdog or upperdog. Partisanship represents a person's connection with other people and a manifestation of their ideological orientations directed toward government policy. The two are inextricably intertwined. Yet they are not necessarily fungible in judicial decision-making models. Both ideology and partisanship are significant in this model of reapportionment decisions in district courts.

The special partisan nature of reapportionment plans makes them an available test for this analysis. Although the reapportionment cases were specifically chosen due to the political and partisan implications, decisions on economic and other civil liberties cases may also be influenced by reference-group considerations. State legislatures pass laws that frequently find their way to court, and some of this legislation is enacted along party lines. In future research, a control for reference-group attachment may prove useful in describing the influences on decisions. A control for partisanship should be considered where it is logically plausible to assume that a judge knows that a particular party has had the upper hand in crafting legislation.

Notes

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1. Recent literature questions the usefulness of reapportionment for advancing the interests of a particular party (Born 1985; Niemi and Winsky 1992), but these studies are counter to the conventional wisdom of the period under investigation.

2. Prior to this decision, district courts allowed large population deviations in one house of bicameral state legislatures, so long as one house was equally apportioned.

3. Once again, these percentages refer to the nominal percent deviation of a single district from population parity, in this case a district with 15% *fewer* people than exists in an ideal district versus a district with 15% *more* people than an average-sized district.

4. See *Gaffney* v. *Cummings* 1973, *White* v. *Regester* 1973, and *Chapman* v. *Meier* 1975. *Mahan* v. *Howell* (1973) set an acceptable limit of 16.4%. In 1983, the Court explicitly allowed a 10% variation in reapportionment cases while upholding a plan with an 89% variation in a Wyoming case for reasons of rational state policy (*Brown* v. *Thomson* 1983). See Redenius 1982 for a further discussion of the cases in the early 1970s.

5. Unlike state legislative districts, the population guideline for congressional seats requires exact population equality, as outlined by Art. I, Sec. 2 of the Constitution (*Westberry* v. *Sanders* 1964).

6. Such were *Gunderson* v. *Adams* (1970), a Dade County, Florida multimember-district case and *Chavis* v. *Whitcomb* (1969), dealing with Marion County, Indiana.

7. An example is the South Carolina case *O'Shields* v. *McNair* (1966), in which the apportionment plan was found lacking but no time was available before the next election for the legislature to fix the plan. Therefore, the court temporarily allowed the old plan to be used.

8. This requirement is found at 28 U.S.C. 2281, 2284. Also, in reapportionment cases, at least one judge is from the court of appeals. Therefore, one-third of the judges in this study are from the intermediate courts. No impact from this is expected or controlled for.

9. One case coded from the *Federal Supplement* recorded a single judge's vote decision, and another reported four.

10. The partisan identification of four judges could not be determined and they were excluded from the analysis. Since the partisanship could not be determined from the multiple source used, I assumed that these judges were apolitical and therefore there was neither a reference-group attachment nor a partisan identification to be used.

11. For census enumeration, see *Montana* v. *U.S. Department of Commerce* (1991); for political gerrymandering, see *Republican Party of Virginia* v. *Wilder* (1991).

12. The state was the defendant in each case. The plaintiffs were all challenging the reapportionment plan and were of various—and usually undeterminable—backgrounds.

13. Partisan affiliations were also found in data provided by Donald Songer and Robert Carp from work they had done previously.

14. Nebraska and Minnesota legislatures were nonpartisan during this period and are not included in this analysis.

15. Not all states have two-thirds override, but those with different limits have legislative majorities that overcome any veto level.

16. In 76 decisions the judge was of the same party as the dominant legislative party that created the apportionment plan; in 63 observations, the judge was of the opposite party; and 57 decisions were neutral due to a lack of legislative control in drawing the plans.

17. Multicolinearity between the reference group and ideology variables is minimal. The reference group variables are removed from the party-label variable and indicate not the partisanship of the judges but the interrelation of the judges with the legislatures: the reference-group similarity, not the partisan identification of the judges, is assessed in the same- and different-party variables.

18. The states considered to be southern include Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

19. In *Chapman* v. *Meier* (1974), the fact that the Missouri River cut the state into two unequal parts was the reason given by the district court for allowing a 20.14% deviation.

20. For instance, see Songer and Haire (1992) for a discussion of a variable of this type regarding First Amendment claims in obscenity cases.

21. Logit analysis does not lend itself to controls for autocorrelation (Aldrich and Nelson 1984, 81), so the single-time-period distinction is included in an attempt to control for the logical impact from the settling of precedent.

22. A separate ordinary least squares regression analysis provided an adjusted R-squared of .50.

23. See Segal and Spaeth 1993, 364–372, for a discussion of logit impacts.

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LIFE-CYCLE TRANSITIONS AND POLITICAL PARTICIPATION: THE CASE OF MARRIAGE

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We investigate the consequences of changes in marital status for political participation, treating marital status as marking points of continuity and transition in an individual's life history and marriage as a setting that fosters interaction and interdependence between marital partners. The analysis is based on panel and pseudopanel data from the 1965–82 socialization study of parents, offspring, and spouses. We find that marital transitions affect participation in four ways: (1) marital partners adjust their activity levels to become more like each other after marriage; (2) marital transitions of any type, especially marriage among younger people, tend to depress participation; (3) the overall effect of marriage, however, is powerfully mediated by the participation level of the partner; and (4) these mediation effects are greatest for political activities that involve collective efforts or draw upon the couple's joint resources.

After languishing for some time in the onslaught of individually oriented inquiries on the one hand and large-scale aggregate analyses on the other, the study of the intermediating roles played by networks and contexts in American political life has more recently enjoyed a boom period (e.g., Eulau 1986; Huckfeldt 1979, 1986; Huckfeldt and Sprague 1987; Kenny 1993; MacKuen and Brown 1987; Weatherford 1982). Curiously enough, the most fundamental component of an individual's social network, the family, has received relatively little of that attention. When the family has been included, it has been treated in broad fashion, with scant attention being paid to the dynamics involved or the complex nature of the influence relationships.

Yet it has been demonstrated, for example, that members of one's family are the most frequently named discussion partners for important problems in general (Marsden 1987); that spouses are the most frequently cited main political discussion partner (Beck 1991); that the presence of a spouse for politically interested individuals provides a strong boost to political conversations (Straits 1991); that husbands and wives have moderately similar participation profiles (Jennings and Niemi 1971); and that family members, including spouses, are among the most frequent targets of interpersonal political persuasion attempts during the election season (Michigan 1968). One of our major objectives is to situate the husband-wife dyad in the larger domain of how social networks affect political behavior.

We draw upon arguments about the new circumstances and social networks introduced by marriage to investigate the consequences of changes in marital status for political participation. Despite its importance in the everyday lives of most adults, the role of marital status as it affects participation has not been well understood. In part, this state of affairs stems from treating marital status in a static fashion and ignoring the potential contribution of spouses. Marital status, however, reflects a dynamic and interactive process. Being married (or partnered) is preceded by being single; all but a handful of marriages and partnerships end in separation, divorce, or the death of a spouse, thereby producing at least a temporary period of singleness; and some individuals always remain single. Recent decades have witnessed an increasing proportion of individuals with complicated marital histories. Moreover, even long-lived marriages are marked by evolutionary tendencies. In short, marital transition rather than marital stasis is the more appropriate vantage point.

There are two prime ways in which changes in marital status could affect the modes and levels of participation. One way emphasizes how participation levels may be directly affected by the "life-space" changes brought about by marital transitions. These include the more practical consequences of marriage, divorce, or widowhood, such as the general dislocations from previous modes and styles of living; the changes in daily life, employment and occupational endeavors, and place of residence that often accompany marriage and its early years; and the demands and rewards of joint responsibilities and opportunities that typically engage husbands and wives. Some of these changes in circumstances and priorities may serve to enhance participation, others to dampen it.

The second perspective focuses on the effects of changing marital status as they are mediated by the particular characteristics of one's spouse. Marital transitions alter the nexus of social forces affecting the participation decision by joining or separating two people with their own unique political traits and histories. The entry into marriage, in particular, creates new opportunities for husbands and wives to learn from and influence each other and thus either to encourage or discourage political involvement on the part of the other. So understood, the implications of marriage for political participation must be specified by considering the direct and indirect interpersonal influence exerted by the spouse.

In sum, marital transitions might introduce changes in political participation either directly as a consequence of the altered circumstances in which individuals live their lives or indirectly through the operation of influence processes in reconfigured social networks. Our inquiry into the impact of marital transitions on political participation will consider both mechanisms of change.

METHODS

Assessments of the connection between political participation and life histories have typically relied on cross-sectional approaches. While satisfactory for some purposes, these approaches suffer from the lack of information about levels and modes of participation before and after particular life history events. Instead, statistical controls are introduced in order to establish ceteris paribus conditions. That has been almost universally the case when scholars have assessed the effects associated with marital status (e.g. Petrocik and Shaw 1991; Teixeira 1987; Wolfinger and Rosenstone 1980). Subjects are classified according to current marital status and then analyzed in terms of recent political participation, holding other factors constant. Much can be learned by this approach, but the presence of before-and-after measures, control groups, and extended participation histories would supply a more powerful set of tools for examining the consequences of marital status and transitions.

Moreover, the question of spousal influence with respect to participation has been infrequently addressed. When it has been, the focus has usually been on the related but distinct topic of spouses as discussion partners and sources of information or has been limited to the topic of voting (e.g. Beck 1991; Straits 1990, 1991; but see Kenny 1993). Further, respondent reports about the spouse have typically been used, reports that range all the way from reasonably reliable ones on such hard indicators as vote turnout and preference to much less reliable ones about less concrete expressions of attitudes and behaviors. And again, the dynamic element is usually absent.

This investigation utilizes the national, three-wave study of high school seniors and their parents that began in 1965, with follow-ups in 1973 and 1982. The design and execution of the study are described elsewhere (e.g., Jennings and Markus 1984). The crucial body of data for present purposes consists of the sample of marital couples generated from the 1973 and 1982 surveys by combining the personal interviews of the primary respondents with the self-administered questionnaires of their marital or (in a few instances) cohabiting spouses. Spouses who were at home were asked to complete a self-administered questionnaire while the primary respondent was being interviewed; those not at home were requested to fill it out later and mail it in. All things considered, the response rates were rather high. For 1973 the figures were 71% (respondent N = 659) and 75% (N = 556) for the parental and offspring generations, respectively. Figures for 1982 were slightly lower, 65% (N = 439) for the parents and 59% (N = 515) for their offspring.[1]

The participation measures were constructed from responses to detailed questions about the respondents' political participation in the 1965–73 period, obtained in the 1973 survey, and for the 1973–82 period, obtained in the 1982 survey. Standard questions about electoral participation replicated those from the National Election Surveys and included seven activities: (1) trying to persuade people how to vote; (2) attending meetings, rallies, and demonstrations; (3) doing other work for a party, candidate, or issue; (4) wearing a button or using a campaign sticker; (5) giving money; (6) voting in the most recent presidential election; and (7) voting in the next most recent presidential election. Four that are not necessarily related to the electoral process included (1) writing a letter or talking to public officials; (2) writing a letter to a newspaper or magazine; (3) taking part in a demonstration, protest, or strike; and (4) working with others to solve community problems.

THE CONVERGENCE EFFECTS OF MARRIAGE

Our first foray into the consequences of marriage looks at the adjustment and accommodation in participation one would expect to occur among couples rather early on in their marriages. Each partner brings to the marriage his or her own political participation history. These histories and emerging patterns may be similar or quite dissimilar, heavily or sparsely laden with acts of participation. Because marriage gives rise to a new and shared set of social and economic circumstances and because it opens up extensive opportunities for the newlyweds to learn from and influence each other, one would expect that the adjustments occurring would result in higher consonance after marriage. The key question, then, is whether marital partners adjust their behaviors so that they act in a more unified fashion than they did prior to marriage.

This question can only be investigated by an analysis of the youth cohort, because the great majority of the parental sample had been long married even by 1965. Two analytic designs will be used, both of which are feasible because of the extended participation histories developed for the primary and spouse respondents. Each takes advantage of two dates: the year in which the couple married and the year in which each partner reported having participated in a given political activity. In the first design, we compare the agreement between partners who were already married (as of the time frame being observed) versus eventual partners who were not yet married. In the second design, we compare the agreement

TABLE 1

Self-Spouse Correspondence among Present and Future Couples

MARITAL STATUS AS OF ELECTION DATE	1968	1972	1976	1980
	VOTING			
Not yet married	.09 (297)	.44 (29)	.04 (81)	.47 (32)
Already married	.36 (182)	.66 (450)	.54 (432)	.56 (481)
	OTHER POLITICAL ACTIVITIES			
Not yet married	.03 (249)	.29 (25)	−.04 (57)	.01 (25)
Already married	.10 (151)	.35 (375)	.13 (305)	.14 (337)

Note: Entries are Pearson correlation coefficients with Ns in parentheses. Each analysis is based on the sample of 3-wave respondents for whom spouse data were available for the relevant election; the 1968 and 1972 analyses make use of the 1973 self–spouse pairs, and the 1976 and 1982 analyses make use of the 1982 self–spouse pairs. The Ns for the nonvoting activities are smaller than in the case of voting because of missing data on the date of participation.

between partners both antecedent to and subsequent to their union. Readers familiar with Campbell and Stanley will recognize the combination of these two designs as an extension of one of their "patched up" designs, wherein cross-sectional and longitudinal approaches are joined together (1963, 56–61).

We turn first to an analysis of *couple correspondence* on voting turnout across four presidential elections (1968–80), comparing couples already married as of any given election year and those not yet—but destined to be—married (Table 1). This analysis employs the three-wave primary respondents and their spouses for whom data are available in either 1973 or 1982. For any given election we have a set of marital partners who reported on whether they voted in that election. Some of these partners were already married by the time of the election being reported on; others would eventually be married to each other. This means that the number of couples who are classified as premarital and postmarital varies by election year. Using the 1968 election as an example, 297 and 182 represent the total number of couples for whom we have voting data. The coefficient of .09 represents the correspondence on turnout for couples who would eventually marry but were not married as of the election date.[2] By the same token, the .36 coefficient represents the similarity of couples who were already married by the time of the election. Results for the remainder of the election years were arrived at in similar fashion.

Across the four elections, similarity in turnout is demonstrably higher in each year among couples who were already wed compared with those "destined" to be wed. Inferentially, then, the fact of being married, with its attendant sharing of a common ambience, desire for marital harmony rather than disharmony, and perhaps the sheer practicality of casting a vote, works to make contemporaneously married couples more alike than those who would later come to be married.

Voting is the most ubiquitous form of participation; and even though it is an act undertaken alone, the decision to vote and the actual act of voting are often done in couple fashion, as a casual inspection of voting lines suggests. Do we observe the same movement toward marital partner correspondence when looking at other, more demanding acts of participation? We can evaluate this question because respondents reported whether—and if yes, when—they had engaged in five additional political activities: influencing others, contacting public officials, working to solve local problems, writing a letter, and protesting. In order to facilitate comparisons with the voting analysis and because participation of almost any kind peaks in the election years, we restrict the analysis here to participation undertaken during the four election years embraced by the study. A summary index (0–5) was constructed for each year based on whether the respondents had performed the given activities and subsequently collapsed into a three-category measure for analysis.

Again, we calculated the correspondence between marital partners according to whether the marriage had occurred prior to or after the year in question. As the bottom half of Table 1 shows, the pattern is quite similar to that for voting turnout. "Couples to be" were demonstrably less consonant than "couples in hand" in every election year. Thus marriage seems to promote unison on more than simply the highly salient and frequently practiced act of voting.

At the same time, it is quite apparent that voting occupies a unique place in the participation repertory. Couple congruence on the activities index, while evident after marriage, falls well short of that obtained for voting. As a highly salient, discrete act with a short, fixed window of opportunity, voting differs appreciably from the nonvoting activities. It is also a participation opportunity that virtually requires a response, since a heavily charged stimulus is there for everyone: a decision to vote or not to vote must be made. Not so for the other activities, which not only require more commitment and self-initiated behavior but also do not demand a response to invitations which are far more idiosyncratic than the invitation to vote. All of these factors combine to increase the likelihood that the decision to vote will be a joint decision.

Impressive though the effects of marriage appear to be, with a design of the type just employed it is conceivable that self-selection effects are at work. Perhaps there is something about people who married earlier that pushes them more quickly toward symmetry in their participatory behavior. While no definitive rebuttal can be offered to this alternative, two pieces of evidence seriously weaken that argument. One is the strength and consistency of the results. The effects are indeed present throughout, even among couples who married as late as just prior to the 1980 election, when most of the respondents would have been at least 30 years old. By virtue of essentially replicating the design over time we have strengthened the plausibility of the findings. A second counterargument has to do with the corollaries of early marriage. Education is the variable most strongly related to date of (first) marriage, with the least-educated marrying earlier. But education is poorly related to marital symmetry on participation, so it seems highly unlikely that self-selection effects could spring from that source.

In any event, the results can be further strengthened and elaborated upon by using another design feature of the study. This second, complementary approach employs a pseudopanel design and observes the same couples over time. A crucial subset of couples meets the requirements for the classic before-and-after configuration, with the two observations of turnout correspondence flanking the intervention or "treatment" of marriage. As before, we divide the results between voting and other forms of participation, but now we must take into more precise account the actual dates of marriage so that they can be calibrated to cover the pre and post observations.

TABLE 2

Self-Spouse Correspondence among Couples over Time

VOTING

GROUP	1968	1972	GROUP	1976	1980
Married in:			Married in:		
1969–1972 (268)	.14	.62	1977–1980 (49)	.00	.69
1965–1968 (182)	.36	.69	1965–1976 (432)	.54	.53

OTHER POLITICAL ACTIVITIES

GROUP	1968	1972	GROUP	1976	1980
Married in:			Married in:		
1969–1972 (224)	.04	.37	1977–1980 (32)	−.15	.37
1965–1968 (151)	.10	.31	1965–1976 (305)	.13	.12

Note: Entries are Pearson correlation coefficients with Ns in parentheses. Each analysis is based on the sample of 3-wave respondents for whom spouse data were available for the relevant election; the 1968 and 1972 analyses make use of the 1973 self–spouse pairs, and the 1976 and 1982 analyses make use of the 1982 self–spouse pairs. The Ns for the nonvoting activities are smaller than in the case of voting because of missing data on the date of participation.

Consider, first, the act of voting, in the top half of Table 2. The four columns show the correlations in turnout among couples according to when the marriage occurred. Two groups, shown in the first row, easily comprise the most appropriate and "cleanest" marriage cohorts for testing the convergence hypothesis. For the 1968 and 1972 elections, the appropriate group is the 1969–72 cohort, couples who were not married as of the 1968 contest but who were married by the 1972 competition. Similarly, for the 1976 and 1980 elections, it is the 1977–80 cohort, whose marriages were sandwiched between those two elections. In each instance becoming married accompanied a stunning increase in turnout concordance. This extraordinary shift between the "before" and "after" observations could hardly be more supportive of the convergence argument.

Although it scarcely seems plausible that some sort of historical or generational effect was at work making all couples more similar over time, that alternative can be tested by examining two other marriage cohorts, those shown in Table 2, row 2. These cohorts, already married at the time of the pair of elections in question, constitute control groups of a sort. No "before" observations are available here; yet on the assumption that convergence on voting sets in rather quickly after marriage, these couples should register much smaller gains across successive elections than in the case of the true pre/post couples.

As the coefficients reveal, that is precisely what happens. The moderate increase registered by the early-marrieds of the 1965–68 cohort almost surely stems in part from the fact that 1968 represented their very first opportunity to vote in a presidential election; indeed, for around a tenth of them that opportunity did not come until 1972. Faced with the problematics of a new stimulus and opportunity, these couples nevertheless achieved considerable agreement by 1968 and then moved up another notch by 1972. Even stronger confirmation of early convergence is provided by the cohort consisting of all couples who had married in the 1965–76 period. These couples, whose length of marriage averaged 12.1 years as of 1982, achieved absolutely no gain between 1976 and 1980. Including the 1976 election, most of these couples had experienced at least two trial runs by that time and three by 1980. They had had time to develop a more or less set response to the stimulus of an election.

As with voting, so too with other kinds of political acts, as shown in the bottom half of Table 2. Again, the two critical pre/post cohorts experienced a sizable gain in congruence over time. These gains are quite impressive when laid against the gains for the two control groups, couples who were already married by the 1968 and by the 1976 election. The former became modestly more similar between 1968 and 1972, whereas the correspondence of the latter did not change over the 1976–80 period, findings that parallel the voting results.[3]

Following either design strategy, then, the evidence is unequivocal. The transition into marriage spurs a process of adaptation that results in greater couple correspondence after marriage. Whether because husbands and wives find themselves newly sharing a common set of resources, opportunities, and constraints or because of the learning and exchange that takes place in new marriages, the result is an enhanced similarity in their participation tendencies relative to the similarity in place when their marriages began. The transition to marriage, with the adjustments it entails, clearly reshapes individual patterns of participation.[4]

MARITAL TRANSITIONS, SPOUSAL INFLUENCE, AND RATES OF PARTICIPATION

Up to this point we have concentrated on how marriage tends to bring marital partners closer together in terms of their participation profiles. This tendency emerges from the influence processes created by the contexts and social networks formed and sustained by the marital union. While assortative mating undoubtedly contributes to husband-wife similarity, our evidence points decidedly toward the key role of postmarital adjustments as well. In this sense, we have seen how the predispositions brought into the marriage, built up from earlier political learning, are often modulated in light of the new circumstances and influences that the newly married face.

These results shed much light on the consequences of a change in marital status but leave open a key question about the mechanism of change: Do husbands and wives become more similar because of influence that they exert upon each other or because their participatory behavior is being shaped by an environment that they have newly come to share? The convergence results also fail to reveal whether marital transitions have an elevating or depressing effect on the rates of participation and whether these effects differ according to the mode of participation. Our attention now turns to these two topics.

Although previous studies have not evaluated the effects of transitions into and out of marriage, the general question of whether marital status affects participation levels, particularly voting, has been addressed in the literature. Most studies have shown that, if anything, married people vote at rates that are modestly higher than those of unmarried people, after controlling for other demographic and socioeconomic factors (e.g., Kingston and Finkel 1987; Petrocik and Shaw 1991; Teixeira 1987; Wolfinger and Rosenstone 1980; see also work from earlier decades cited in Milbrath and Goel 1977, 115). The most frequently offered explanations of this finding emphasize the role of marriage in encouraging stability in one's personal life and in fostering integration into one's community.

However, once these explanations are considered in the context of an explicit focus on marital transition (the entry into or dissolution of marriage), it is clear that one must distinguish between the long and the short run. Marriage may promote such stability and integration in the long run; but in the short run, marriage, like widowhood or divorce, should usher in *less,* not greater stability in one's personal life; should deflect attention away from political affairs as personal preoccupations take hold; and should thus act to discourage, not encourage participation. Findings about marital status and age reported by Wolfinger and Rosenstone (1980) are suggestive in this respect. They found that the turnout gap between the married and the unmarried rose steadily with age.

The effects of marriage will also be felt through the mechanism of spousal influence. We conceive of spousal influence as operating in two ways, which together generate a set of expectations about how it will be manifested for individuals at different stages of their marriages and across different modes of participation. First, spousal influence arises as marriage alters the learning environment of the individuals affected. The shared interactions of husbands and wives should prompt a process of learning that reduces their differences when it comes to the beliefs and predispositions that sustain or undercut political participation. Although these effects should be evident across a whole array of participatory acts, spousal influence in this sense should be greater in the early years of marriage. The newer the marriage, the greater the learning that is taking place with respect to a variety of domains (including the political) and the lower the likelihood that any past influence is already reflected in the subject's own participation history.[5]

Second, spousal influence alters the context of decision making when it comes to activities that either involve collective efforts or make use of the couple's shared resources. Social or collective activities, such as working to solve community problems, become activities that husbands and wives can undertake together; when faced with any particular opportunity to participate, one partner may exert influence upon the other to join in. Other activities requiring the engagement of the couple's joint resources encourage partners to confront their differences and reach agreement. For example, husbands and wives typically share front lawns (and thus the display of lawn signs), automobiles (and thus the display of bumper stickers), and checking accounts (and thus contributions to parties or candidates). Consequently, while influence derived from learning processes should be evident across the full array of participatory activities, the influence of the spouse should be enhanced for modes of participation that involve collective efforts or that draw upon the couple's joint resources.

The expectation that spousal influence effects will be enhanced for social or joint activities parallels the arguments of others who have distinguished between socially based and individually based acts when studying the effects of nonfamilial discussion networks and social contexts (Giles and Dantico 1982; Huckfeldt 1979).[6] Our classification scheme, however, differs from previous schemes in that we group acts based on the degree to which they appear to require interaction, cooperation, or consensus between marital partners. Under the rubric *social or joint activities* we place working with others on a local problem; attending a meeting, dinner, or rally; giving money to a party or candidate; and displaying a bumper sticker or button. Our category of *individual acts* consists of contacting public officials, writing a letter to an editor, trying to influence others' votes, and engaging in other party activity.[7] In addition, we treat *voting* as a separate category. Although nominally an individually based act, voting is also a case (as we pointed out earlier) where one would expect strong spousal influence effects to be operating (see Kenny 1993; Straits 1991).

In order to assess the effects of marital transitions on participation levels and to evaluate these expectations about spousal influence, we estimated a set of regression models designed to explain changes in participation levels between the 1965–73 and 1973–82 periods, again taking advantage of the longitudinal component of the design. Each model was estimated separately for the youth and parent generations and replicated for indices of individual acts, social or joint acts, and voting. Because a change-score dependent variable does not accommodate floor and ceiling effects, we use 1973–82 participation levels (as reported in 1982) as the dependent variable and 1965–73 participation levels (as reported in 1973) as an explanatory variable. The remaining set of variables help account for any change or discrepancy in the respondents' activity levels between the 1965–73 and 1973–82 periods.[8] These variables fall into three categories that we will describe in detail: (1) *marital transition* variables, which will capture any direct effect of changes in marital status; (2) *spouse participation* variables, which will reveal the spousal influence effects; and (3) *control variables*, reflecting personal characteristics and life circumstances that are not of direct interest to us but may work to alter participation levels and introduce spurious similarity into husbands' and wives' participation profiles.

The marital transition variables were based on responses to a detailed set of marital history questions asked of the primary respondents. The overwhelming majority of the parents underwent no change in marital status across the 1973–82 period and fell into one of three key categories: (1) those continually married during that time, (2) those widowed during that period, and (3) those already widowed or divorced in 1973 and whose marital status did not change up to 1982. In contrast, the patterns of marital transition among the younger respondents were many and varied. Aggregating across this diversity, we developed four summary groupings: (1) those married to the same partner throughout the 1973–82 period; (2) those married to a new partner as of 1982, whether a first marriage or not; (3) those married in 1973 who were widowed, divorced, or separated by 1982, regardless of whether multiple transitions occurred in the interim; and (4) those not married in either 1973 or 1982, the great majority of whom were unmarried across the entire 1973–82 period. The marital transition groups were represented in the model by a series of dummy variables, with the unmarried group serving as the excluded category.[9]

Variables referring to the spouses' participatory behavior were fashioned to parallel those constructed for the primary respondents. Because we view the influence process as contemporaneous, we included the spouse's 1973–82 participation level as a predictor for the continually married and the newly married (youth only) respondents. Spousal influence effects were estimated separately for each of these groups. When it comes to those widowed or divorced during the 1973–82 period, we incorporated the (ex-)spouse's 1965–73 participation level. This specification captures the possibility that a residual of influence remains after the marriage ends.

The set of control variables included the respondent's level of education, economic achievement, and employment status (as of 1973), as well as the change in these indicators between 1973 and 1982,[10] the number of residence changes between 1973 and 1982, the number of children born between 1973 and 1982 (youth generation only), the number of children born in the pre-1973 period (youth generation only), the number of young children (<18) and number of older children (18+) in the home as of 1973 (parent generation only), age and age-squared (parent generation only), and sex. The effects of all of the parental status variables were estimated in interaction with sex. Of course, the newly married and newly divorced or widowed were undoubtedly undergoing a whole series of life events attendant on their marital transition—changes in economic status, residence, parental status, and so on. This analysis does not try to trace out these resulting changes in life circumstances and their contribution to changes in political involvement but simply controls for such changes, revealing any direct effects of marital transitions that remain.[11]

Before turning to the results, the special nature of these samples is worth underscoring. Neither sample, of course, is representative of the general population. Each is characterized by a profile of characteristics, including marital attributes, that draws its main features from the sampling design of the original study (detailed in Jennings and Markus 1984, among other places). All parental respondents were at one point married and had at least one child (who was finishing high school in 1965); and they were on average entering their sixty's by 1982. The youth respondents, in turn, are an age-homogeneous and relatively highly educated group, fully bearing the patterns of marital transition that have come to characterize contemporary times. Working with such samples requires cautious generalization; but as we have already seen and will see again, offers special opportunities as well. The youth sample, in particular, is uniquely well suited to speak to questions about the entry into marriage. Furthermore, the quasi-experimental logic of the longitudinal analysis helps guard against some of the problems of inference that would threaten a cross-sectional analysis of these or other data.

Tables 3 and 4 contain the key results from the regression analyses for the youth and parent generations, respectively.[12] For each dependent variable we present estimates from two models. The baseline model takes into account marital transitions but ex-

TABLE 3

Marital Transitions, Spousal Influence, and Political Participation: Youth Sample

INDEPENDENT VARIABLES	ACTIVITY LEVEL 1973–82				VOTE 1980	
	INDIVIDUAL		SOCIAL/JOINT			
	S.P. EXCL.[a]	S.P. INCL.[b]	S.P. EXCL.	S.P. INCL.	S.P. EXCL.	S.P. INCL.
Constant (unmarried 1973–82)	.89	.88	1.03	1.03	.66	.68
1965–73 activity level	.40****	.37****	.36****	.32****	.30****	.24****
	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)
Married throughout 1973–82	−.25**	−.40***	−.24*	−.55****	−.01	−.29****
	(.13)	(.14)	(.16)	(.16)	(.05)	(.06)
Spouse's 1973–82 activity level	—	.21****	—	.34****	—	.36****
		(.05)		(.05)		(.06)
Newly married 1973–82	−.38***	−.45***	−.26**	−.53****	−.09**	−.35****
	(.13)	(.15)	(.15)	(.16)	(.05)	(.07)
Spouse's 1973–82 activity level	—	.10*	—	.27****	—	.38****
		(.07)		(.07)		(.07)
Divorced/separated between 1973–82	−.22*	−.29*	−.18	−.22	−.13**	−.18**
	(.16)	(.19)	(.19)	(.21)	(.06)	(.08)
Spouse's 1965–73 activity level	—	.11	—	.08	—	.10
		(.15)		(.16)		(.13)
Adj. r^2	.23	.25	.19	.26	.18	.26

Note: Number of cases = 630. Entries are unstandardized OLS regression coefficients with standard errors in parentheses. The dependent variables are 1973–82 individual activity level (coded 0 to 4), 1973–82 social/joint activity level (coded 0–4), and 1980 vote (coded 0 or 1). The marital transition variables are coded as a set of 0/1 dummy variables, excluding the unmarried group. In the individual and social/joint activity analyses, the respondent's past activity level and the spouse's activity level were coded to range from 0 to 4. In the voting analysis, the respondent's own vote history took into account the 1968 and 1972 elections, coded 0 (voted in neither), .5 (voted in one), and 1.0 (voted in both). For the formerly married, the spouse's vote variable also took into account the 1968 and 1972 elections and was coded similarly. For the currently married, the spouse's vote variable was their 1980 vote (coded 0 or 1). Each equation also included a number of control variables, as described in the text, that were coded to mean zero.

[a]Spouse participation excluded.

[b]Spouse participation included.

$^{*}p < .10$.

$^{**}p < .05$.

$^{***}p < .01$.

$^{****}p < .001$ (one-tailed).

cludes the spouse participation variables (cols. 1, 3, 5). Findings from this model indicate the effects of marital transitions, in and of themselves, on participation levels. The second stage of the analysis incorporates the spouse's participation profile into the model (cols. 2, 4, 6). When considered in light of the baseline model, this phase of the analysis reveals how conceiving of marital transitions as movements into new social networks enriches our sense of the implications of marriage for political involvement.

In the equations that exclude spouse participation levels, the coefficients associated with the *married throughout*, *newly widowed/divorced*, and *newly married* (youth only) groups indicate the estimated participation differences between the named group and those who were unmarried in 1973 and in 1982 (the excluded category), net of all the other variables in the model.[13] For example, Table 3, column 1 contains a coefficient of −.38 ($p < .01$) for the group of young adults who married during 1973–82. This means that the newly married youth were estimated to have participated in individual activities at a rate that was substantially lower (by .38) than that of the unmarried youths who were otherwise similar in their participation histories, personal circumstances, and individual traits. Expressed in terms of change, this result indicates that, ceteris paribus, the young adults' entry into marriage had the effect of depressing their participation.

A perusal of the findings for the younger generation reveals that, if anything, transitions into and out of marriage tend to dampen participation levels. Youth who experienced a divorce or separation tended to participate at lower levels than comparable respondents who were unmarried across the period, but the group contrast attains statistical significance only for the case of voting. The findings are stronger and more consistent with regard to the youth who underwent the transition into marriage. Becoming married substantially lowered participation rates with respect to each participatory mode, though the largest effect here arises with respect to individually based acts. Finally, even the young adults who were married throughout the 1973–82 period showed a decrease in their rates of participation relative to the unmarried group, though the effects are limited to the more demanding political activities and are somewhat more modest than those evident for the newly

TABLE 4

Marital Transitions, Spousal Influence, and Political Participation: Parent Sample

INDEPENDENT VARIABLES	ACTIVITY LEVEL 1973–82					
	INDIVIDUAL		SOCIAL/JOINT		VOTE 1980	
	S.P. EXCL.[a]	S.P. INCL.[b]	S.P. EXCL.	S.P. INCL.	S.P. EXCL.	S.P. INCL.
Constant (unmarried 1973–82)	.45	.45	.44	.47	.15	.22
1965–73 activity level	.45**** (.03)	.43**** (.03)	.48**** (.03)	.41**** (.03)	.54**** (.03)	.48**** (.03)
Married throughout 1973–82	−.12 (.09)	−.18** (.09)	−.09 (.11)	−.34**** (.10)	.00 (.03)	−.30**** (.05)
Spouse's 1973–82 activity level	—	.11*** (.04)	—	.29**** (.03)	—	.34**** (.04)
Widowed during 1973–82	−.22** (.13)	−.25** (.15)	−.11 (.15)	−.28** (.17)	.04 (.04)	−.24** (.14)
Spouse's 1965–73 activity level	—	.05 (.11)	—	.23** (.12)	—	.30** (.14)
Adj. r^2	.40	.40	.40	.47	.50	.55

Note: Number of cases = 570. Entries are unstandardized regression coefficients with standard errors in parentheses. See the note attached to Table 3 for a description of the variables and their coding. Each equation also included a number of control variables, as described in the text, that were coded to mean zero.

[a] Spouse participation excluded.

[b] Spouse participation included.

*$p < .10$.

**$p < .05$.

***$p < .01$.

****$p < .001$ (one-tailed).

married youth. This group did not, of course, undergo a change in their marital status across the period, but they were still moving through the early stages of their marriages. Viewing the results for this group together with those for the newly married youth suggests that the adjustments taking place *throughout* the early years of marriage serve to depress political participation.

Turning to the parental generation, we again see signs that marital transitions may work to discourage participation; though the evidence here is less persuasive (Table 4). Recall that the parent-generation equations contrast respondents who were either widowed or continually married during the 1973–82 period with those who already divorced or widowed by 1973 (the "unmarried" baseline group). The newly widowed parents' individually based participation rates were depressed relative to those characterizing the model's baseline group, but this dampening effect does not extend to the other participatory modes. Moreover, the parallel finding for the continually married parents is also negative, albeit weaker in magnitude and not statistically significant.[14] Thus when judged relative to the continually married group (who, unlike the continually married youth, had been long married already by 1973), the newly widowed parents do not emerge as particularly distinctive.

To summarize, these results show that changes in marital status work in the short run to discourage participation when they exert any direct effects at all. Negative effects are evident to some extent for marital transitions of any type, but they are most dramatic for young adults entering marriage (and for individually based acts of participation, a seeming anomaly to which we will return). Marriage may work in the longer run to introduce life circumstances that encourage participation, as previous scholarship has suggested.[15] But our evidence indicates that it first works the other way—by introducing new demands upon the young partners and new priorities into their lives that have the effect of inhibiting, not spurring, political involvement.

This view of how marital transitions affect participation if taken by itself, however, is incomplete. We must also take into account the findings that represent the social-networks perspective. This perspective is represented by the equations that include the spouses' levels of participation, given in Tables 3 and 4, columns 2, 4, and 6. In order to clarify what the estimated coefficients in these columns represent, consider again an example based on the results for the youth generation with respect to individual acts (Table 3, col. 2). The coefficient of −.45 ($p < .01$) for those whose marriages began in the 1973–82 period is not directly comparable to the coefficient from the baseline model (col. 1) because of the inclusion of the interactive spouse participation variables. Here, −.45 refers to the difference between the participation levels of this group and the unmarried (excluded) group, *given a spouse who was fully inactive throughout the 1973–82 period.*[16] The coefficient for the spouse's activity level, .10 ($p < .10$), in turn, indicates the increment in participation associated with a one-act increase in the spouse's participation level. Considered as a pair, these coefficients indicate the extent to which the newly-marrieds' participation rates will be lowered or elevated by the operation of spousal influence, above and beyond the direct effects of marital transitions already described. The comparable coefficients for the other marital groups can be interpreted similarly.

Whether we are speaking of the continually married youth and parents or the newly married youth, there is clear evidence of a process of spousal influence at work. Spouse participation levels introduce sizable deviations from that which would be otherwise expected on the basis of the individual's own personal characteristics, life circumstances, and participation history, as revealed by a comparison with the unmarried group. Those whose spouses were inactive participated at lower levels, and those whose spouses were active participated at the same or higher levels than those who were single. Thus, while the adjustments entailed by a new marriage may work to depress participation levels, the overall repercussions of marriage will also depend upon the involvement patterns and tendencies of one's particular spouse.

Yet it is not simply the case that married individuals will participate at *either* a higher or a lower level than one would otherwise expect. In particular, there is a distinct asymmetry in the possible mobilizing or demobilizing influence of the spouse when it comes to the act of voting, evident for each generational and marital grouping. Of course, given the high level of voting (mis)reported by the respondents as a whole, ceiling effects limit the possible boost that spousal influence might provide. A limited boost from spousal participation is nevertheless evident in each generation (7% and 3% for the continually married and newly married youth, respectively; 4% and 6% for the continually married and newly widowed parents). But a nonvoting spouse, while a relatively rare occurrence, has the effect of lowering voting rates much more substantially than this, by roughly 25–30% in each of these marital groups.[17] There is no reason to consider this drop an artifact. Rather, when set against the field of normative forces encouraging voting and claims of voting even when it never took place, a nonvoting partner carries a very significant weight.

The patterns for those whose marriages ended in the 1973–82 period by widowhood among the parents and by divorce or separation among the youth are distinctive both when compared with those for the married groups and when compared with each other. The key match-up here is cross-generational. There is no evidence of any residual influence among the recently divorced young adults. Though the coefficients are always in the expected direction, they are small and never reach even the .10 level of statistical significance. In contrast, there is some evidence of residual influence among the widowed parents with respect to socially based acts and voting; their rates of participation show adjustments in the 1973–82 period in the direction of greater consonance with the rates previously evident for their spouses.

In one sense, it is not at all surprising that the effects among the parents are found only for social acts and voting; these are the modes where we expect the greatest spousal influence to be taking place. In another sense, however, these results are counterintuitive: the effects are strong for social acts even in the physical absence of the spouse and thus the absence of any direct interaction and exchange. The 1973–82 time period in most cases exceeded the period of the spouse's absence; thus, these effects may partially reflect influence that occurred while the spouse was still alive. We suspect, however, that they also represent later adjustments that reflect the special significance of the marital relationship for this parental group. The generational comparison is instructive in this regard. The marriages of the older widows had typically spanned decades and then ended through the death of the spouse, a traumatic and uncontrollable loss. The youth marriages, on the other hand, were of a much shorter duration and ended, for the most part, by divorce. In light of the marriage's duration, and the circumstances of its dissolution, then, we should not be surprised to find that the parental widows' participation levels bear the trace of residual spousal influence.

A closer inspection of the relative magnitudes of the spousal influence effects across the modes and generations essentially confirms our expectations about the circumstances in which spousal influence should be most powerful. With regard to the marital duration expectations, the suitable cross-generational comparison is between the continually married youth and parents, whose coefficients are as follows:

	Individual Acts	Social/Joint Acts	Voting
Continually married parents	.11**	.29***	.34***
Continually married youth	.21***	.34***	.36***

In each generation, spousal influence effects are appreciably larger for social acts and voting than they are for individually based activities. This is consistent with our expectations about the particular circumstances of marriage and the arguments and findings of others studying the influence of neighborhood context (Giles and Dantico 1982; Huckfeldt 1979; for a caution, see Kenny 1992). Moreover, the effects for individually based acts are larger among the younger-married than among those in the older generation. This, too, is as expected, given the relatively more powerful learning forces at work among the youth.[18] Yet those forces do not result in a clear generational difference with respect to the spousal influence for social acts. There (and for voting as well), the effects are equally powerful among the younger and older married cohorts. When it comes to companionable and joint activities, there is no drop-off in the influence taking place in older marriages or, by the same token, any particular boost in influence among the younger couples.

The variable effects of spousal influence across participatory modes help explain the finding that marital transitions appeared to exert a more powerful dampening effect on participation in individually based activities, as demonstrated in the first phase of the regression analysis. It is not the case that marriage and widowhood act to suppress one's tendency to engage in individually based activities while leaving relatively unscathed one's proclivity to engage in other types of activities. Instead, the dampening effects of those transitions are more pervasive but also more strongly countered by the phenomenon of spousal influence when it comes to social activities. This is most clearly revealed by considering a counterfactual: What if spouses had the same influence regarding individual activities as they do regarding social activities? If so, the average individual activity levels of the newly married youth and the newly widowed parents would not deviate from the unmarried baseline any more than is evident in regard to social activities. Larger direct effects of marital transitions emerge for individually based participatory modes precisely because it is with respect to these modes that the mechanisms of spousal influence are weakest.

CONCLUSION

Previous scholarship has typically treated marital status as a static individual characteristic. In contrast, we have concentrated on the dynamic implications of marital status and on marriage as a setting that fosters interaction and interdependence between husbands and wives. This approach views marital status as marking points of continuity and transition in an individual's life history and focuses on the capacity of changes in marital status to disrupt and reshape past patterns and levels of political involvement. Changes in marital status take on political significance not because they transform the individual's personal characteristics in any immediate or straightforward way, but because they resituate the individual into a new set of circumstances and social networks.

The presence of detailed political histories elicited directly from both marital partners in the youth generation provided vivid evidence for one consequence of this resituating. While assortative mating produced some degree of couple similarity in participation, postmarital adjustments during the early years of marriage generated convergence far exceeding the premarital state. Such convergence serves as a striking but not isolated illustration of how a focus on marital transitions and partners can enlighten our understanding of participation over the life course.

We have distinguished two major ways in which marital transitions may alter the modes and levels of participation. The first is tied directly to the experiences of marriage, divorce, or widowhood. These changes in marital status typically force people to undergo a period of substantial adjustment in their personal lives, both in emotional terms and in terms of their day-to-day life-style. One implication of this disruption in one's personal life (evident in our results) is a disruption in one's political life. All other factors taken into account, participation rates tended to be depressed in the wake of changes in marital status.

The second mechanism focuses not on the adjustments entailed by marital transitions per se but on those that depend upon the participation tendencies of one's spouse. The transition into marriage places the spouse at the center of an individual's social network, creating new opportunities for learning and persuasion to occur. It also creates a relationship of interdependence between husbands and wives, enhancing the likelihood that many participation decisions will be made jointly. Two consequences follow: (1) whether participation levels ultimately rise or fall following the transition into marriage also depends upon the new partner's level of political engagement; and (2) spouses continue to exert influence throughout the marriage's duration, and in the case of long-lived marriages that end in the death of a spouse, even beyond.

Nevertheless, it is worth underscoring the distinctiveness of the early stages of marriage. The disruptive effects of marriage appear to be more potent than those of divorce or widowhood. In our results, the entry into marriage, unlike the exit, reduced one's likelihood of political involvement irrespective of the participatory mode. Moreover, we saw evidence that spousal influence generates the most dramatic shifts in participation rates in the marriage's early years. Thus the entry into marriage is not just a key point of transition in the young adult's personal life-history but a key point of political transition as well.

At the same time, the influence of the spouse is not felt equally across all participatory modes. Our analysis distinguished between individually and socially based modes of participation, following the arguments made by others when studying the importance of neighborhood context, modified somewhat to fit the marital context. Because marriage alters the circumstances in which some participatory decisions are made, as well as providing new opportunities for learning, some activities that might be otherwise classified as "individual" must be considered in "couple" terms. That given, our findings about the distinctive power of interpersonal influence processes with respect to socially based political activities concur with those found in previous research.

Taken together, our results indicate that the conventional wisdom about marriage and political participation should be both extended to recognize the important mediating influence of the spouse and revised to acknowledge the direct, short-term effects of marital transitions. As we have already noted, marriage creates a context of change at the individual level easily missed by simple aggregate comparisons among marital-status groups; the young partners' participation rates are likely to shift (to be elevated or lowered, depending upon the differences that exist between the marital partners) as they learn from and are influenced by their spouses. Marriage also operates directly to alter participation levels; but rather than spurring participation, as the conventional wisdom would have it, marriage, in its early years, appears to discourage participation.

This position and our results are not, of course, flatly incompatible with the semiregular finding that married people in general tend to participate somewhat more often than do the unmarried. As a static generalization that captures the medium-to-long-term effects of being married, this may remain an accurate depiction. Our position and findings do imply, however, that such a generalization must mask an important life-cycle dynamic—variation in the effects of marriage that is tied to the marriage's age. This view receives further support by Wolfinger and Rosenstone's (1980) finding that the married/unmarried participation gap increased with age, a pattern replicated by our own recent cross-sectional analysis of data from the 1992 National Election Study. After controlling for other demographic and socioeconomic characteristics, married people actually voted at *lower* levels than did unmarried people in the younger cohorts, a relationship which was reversed among the older age groups.[19] In short, the life-cycle perspective on marriage, which emphasizes both marital transitions and marital duration, leads to a new and dynamic perspective on how marriage affects participation rates.

Our study also raises a number of further questions and implications. One issue concerns how other alterations in an individual's "life space" compare to marital transitions, both in the significance of their political consequences and in the mechanisms at work. Although much is known about consequences of discontinuities in such domains as residence, employment, and occupation, just how these compare in magnitude and process with those in the marital domain is poorly understood.

Similarly, these findings about the substantial and enduring effects of spousal participation on the partner's level and modes of participation, examined in a dynamic context, raise questions about how the spouse's influence interacts with that of the other actors in an individual's social network. The uniquely intimate nature of the couple dyad may also color the influence process in a way that differs appreciably from that formed by other contacts in the social network.

Moving to a higher level of generalization, our findings have different implications depending upon the changing rates of marital transitions in the society at large. Taken at face value, our results imply that aggregate participation rates would decline as multiple marriages and dissolved marriages increase. For any given individual, a decrease in participation might not be long-lasting; but since the transitions are spread across many individuals and over time, their effects would be cumulative and ongoing. Although far from conclusive, this perspective helps illuminate the possible connection between the decline in particular forms of participation in the United States and the secular trend toward greater discontinuities and varieties in marital histories.

Notes

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1. The fact that the primary respondents among these pairs were personally interviewed, whereas the spouses completed a self-administered questionnaire raises questions about artifacts due to mode of administration. A detailed analysis reveals modest mode effects, at best, for the questions of interest to us here. For a fuller discussion, see Jennings and Stoker 1992; and for reassuring results from a tightly controlled field experiment, see de Leeuw 1992.

2. Pearson correlations are used to measure relative similarity. If gamma (which is less sensitive to distributional skew) is used instead, the findings are even stronger. Column by column, the results are as follows: *voting* .19 vs. .65, .84 vs. .94, .11 vs. .91, and .78 vs. .92; *other activities* .03 vs. .42, .38 vs. .52, −.20 vs. .33, and −.07 vs. .25. If an absolute (distance) measure of similarity is used, patterns similar to those reported in Table 1 are also obtained.

3. Using gamma again yields results that are comparable to those found using pearson Rs. When a measure of absolute agreement is used, the patterns found for voting are comparable, as well. Yet shifts in the marginal distribution of the nonvoting scale (as the youth became more active in general) resulted in greater absolute differences as of the second time point within each pair.

4. This reshaping is reflected in the effects of the early stages of marriage on discontinuities in individual participation levels over time. For example, the correlation between turnout in the 1976 and 1980 election was .54 for individuals who were married before the 1976 election, .31 for those who became married between the pair of elections, and .54 for those who were unmarried across the period. Similar findings are obtained for the 1968 and 1972 elections, as well as for the nonvoting activities in both periods.

5. One would not, however, expect this influence to be restricted to the newly married. Such would be the case if processes of learning and interpersonal influence led inexorably to total husband-wife convergence over time; but despite their shared history, such convergence is implausible. Some differences between partners in their abilities and incentives to participate will persist over time, and new differences will arise as each partner responds to the experiences and influences that characterize their individual lives. In the face of husband-wife differences, whether in the early- or the late-going, the potential for learning remains.

6. For a more skeptical view of the distinction between socially and individually based acts, see Kenny 1992 and Leighly 1990.

7. Displays have been treated as individual acts in previous work, but we reclassify them here in light of the marital context. We considered dropping the "other party activity" item from the individually based index, in light of its miscellaneous character, but analysis demonstrated that doing so did not appreciably alter the results. On the basis of these decisions, we formed two additive indexes, each with four items. One indication that these are valid distinctions lies in

the fact that couple correspondence is somewhat higher for the social/joint acts than for the individual acts.

8. Technically, the coefficients on these variables are identical to those that would be obtained by using a change-score dependent variable, as long as prior participation level is still included as an independent variable to control for floor and ceiling effects. The r-squared values, however, would differ, and would be higher in the alternative specification.

9. Although participation information was gathered in 1973 and 1982, and the marital transitions we singled out occurred between 1973 and 1982, the participation measures pre and post are based on reported participation throughout the 1965–73 and 1973–82 periods, respectively. As a consequence, our estimates of the effects of marital transitions are conservative; they will tend to underestimate the effects on participation levels that would otherwise be revealed by a cleaner pre/post design.

10. The justification for including the respondents' prior level of education and economic status, in addition to the 1973–82 changes in these variables, may not be self-evident. Because the respondent's prior participation level will partly reflect situational forces that were operating in the earlier period, it will only imperfectly capture the stable personal attributes that tend to enhance or depress participation levels. By taking into account the respondents' prior level of education, economic achievement, and employment status, we thus control for factors that, given selection processes, would otherwise tend to inflate our estimates of the spousal influence effects. Economic achievement was summarized by an index combining family income, homeowner status, and subjective class status, each first recoded to range across the same interval and then averaged. Change in education level was only appropriate to (and thus was only included for) the younger generation.

11. Notice also that this model focuses on only one member of each marital couple; it represents one partner's participation level as influenced by the other but not the process of reciprocal influence that is implicit. Ideally, we would want to estimate a model of reciprocal influence that represents this mutual socialization process. Doing so would yield better (consistent) estimates of spousal influence, and would at the same time allow one to evaluate whether there are husband-wife asymmetries in influence. This is a line of inquiry we have pursued elsewhere in an analysis that was necessarily limited to couples for whom we had complete panel data (Jennings and Stoker 1992). In the present context, given our interest in examining the participation rates of those of *varying* marital statuses and histories, the same kind of modeling strategy was not feasible. Explicitly modeling the reciprocal influence process would undoubtedly affect the estimates obtained, though it would be unlikely to lead to conclusions that differ fundamentally from those we draw here—either with regard to the sheer existence of spousal influence effects (given the robust estimates that we will see presently) or with regard to the comparative effects across generations and participatory modes (as it is unlikely that the bias operates differently in these different contexts).

12. The base sample for the regression analyses included all respondents unmarried in 1973 and 1982, those continually or newly married (youth only) for whom spouse data were available in 1982, and those whose marriages ended between 1973 and 1982 for whom spouse data were available in 1973 (parent's base N = 605, youth N = 743). Missing data on the control variables produced further attrition in the sample sizes, resulting in the Ns reported in Tables 4 and 5. The primary source of this attrition were the mailback respondents, because the mail questionnaire did not cover the full series of relevant questions.

13. We opted to present the results from estimating a linear probability model in the voting case, to facilitate interpretation and comparison with the other findings. Accordingly, the voting coefficients can be interpreted in probability terms. A logistic regression analysis yields results that are fully consistent with the ordinary least squares (OLS) results.

14. We have no explanation for the weak negative coefficients for the continually married parents; one would expect them to be indistinguishable from the group of previously widowed/divorced parents who also experienced no marital transitions during the 1973–1982 period. However, we must acknowledge the artificiality of characterizing these groups as undergoing no pertinent transition. The previously widowed or divorced may still have been going through adjustments tied to their marital transition; the continually married parents were going through a late, presumably stable phase of their marital life-cycle. Departures from the ideal of no difference between these groups could easily be tied to such considerations.

15. Given the special nature of these samples, we cannot use these data to help explicate what gives rise to the semiregular cross-sectional finding of a positive relationship between marriage and participation. We will return to this issue in our concluding section and draw upon a cross-sectional analysis of other data to argue that the conventional finding masks a life-cycle dynamic in marriage's effect. Parenthetically, in cross-sectional analyses of the present data, the married youth do not participate at higher rates than the unmarried youth; likewise, the married parents do not participate at higher rates than the parents who were widowed or divorced. These results are not surprising, given our dynamic results and considering the restricted nature of marital status contrasts that they involve.

16. If the spouse participation variables had been coded so that zero represented the *average* spouse participation level, instead of *no* spouse participation, then coefficients comparable to those in column 1 would be obtained. We decided against this coding scheme because it would have made it more difficult to describe the full range of spousal influence effects.

17. The same asymmetry is evident in the results from a logistic regression. To take an example, whereas for the continually married youth the relevant pair of OLS coefficients are −.29 and .36 (each significant at $p < .001$), the relevant pair of logit coefficients are −1.62 and 1.91 (again, each significant at $p < .001$).

18. If the initial phases of marriage introduce a flurry of learning and influence, as we have argued, why do we find weaker estimates of spousal influence for the newly married youth than for continually married youth with regard to nonvoting activities? The likely explanation, especially in light of the results presented in Tables 1 and 2, is that the 1973–82 period exceeds the marital duration of the newly married group. That is, the participation histories for the newly-married's spouses cover the entire 1973–82 period, whereas their marriages did not. Notably, the pattern reverses when it comes to voting in 1980, a point in time when almost all of the new marriages had begun. Here, the effects for the newly married are modestly larger than those found for the youth whose marriages were in place by 1973.

19. To be specific, the analysis controlled for race, hispanic origin, sex, number of young children (<6) and number of older children (6–18), both parental status variables in interaction with sex, length of residence, work status, occupation (professional/nonprofessional), age and age-squared, education, income, and homeownership. Vote was scored 0–1, and the analysis was run separately for respondents aged 18–41 and those 42 or older. In the younger cohort (n = 962) and judged relative to the unmarried group, the (OLS) coefficients were −.056 (t = −1.42) for the married group and −.114 (t = −2.50) for the group of divorced, separated, or widowed respondents. In the older cohort (n = 977), the coefficients were .096 for the married group (t = 1.86) and, oddly, .059 (t = 1.13) for the divorced, separated, or widowed group.

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CONTROVERSY

SOCIAL CONSTRUCTION (CONTINUED)

In this Review *in June 1993 Anne Schneider and Helen Ingram argued that the social construction of target populations is an important political and policy phenomenon. Robert Lieberman criticizes Schneider and Ingram's "circular" conceptualization of public policy and social construction. He proposes a "historical-institutional" framework for understanding the role of group identities in political change. Lieberman analyzes the dual experience of African-Americans in the American welfare state as an example of political institutions and policy changes' affecting changing group constructions. Ingram and Schneider respond that their purpose is to understand how social constructions shape policy designs, which in turn affect citizen perceptions and participation, and argue that Lieberman's ideas of institutions and history yield no analytic improvement. They provide their own analysis of the case of welfare to illustrate the advantages for future research of their conception of policy targets.*

COMMENT

In "Social Construction of Target Populations," Anne Schneider and Helen Ingram present a provocative argument about an issue of fundamental importance to the study of policy formation and development, the effect of the nature of target populations on policy outcomes. They suggest that the allocation of policy benefits and burdens depends not only on the relative social power of groups in society but also on the "social construction" of the groups that policies are intended to affect. Notions of identity have transformed contemporary political theory, and the authors deserve great credit for their attempts to apply ideas about group identity to the study of public policy and to forge a link between the critical and positive traditions in social science. Their work is particularly welcome at a moment when issues of racial, gender, and ethnic identity are at the center of vigorous debate in the political and academic worlds and when the tension between rampant individualism and group solidarity has never been greater (Walzer 1992).

But Schneider and Ingram's proposed approach to this important question lacks a clear causal argument. Its central flaw is the conceptual imprecision of the idea of "social construction," which is one of the pillars of their analysis. The very use of the language of "social construction" begs important questions about the origins and political meaning of group identities. Understanding the importance of the nature of target populations for policy and politics requires an account of where group identities come from, how they change, and how social and political institutions adjudicate among contending definitions of group identity in making public policy. Schneider and Ingram offer none of these.

TARGET POPULATIONS AND POLICYMAKING

The first part of Schneider and Ingram's article sketches a basic claim about the nature of target populations and their impact on policy outcomes. Traditional theories of policymaking account for the allocation of burdens and benefits among social groups on the basis of various measures of social and political power. To this dimension Schneider and Ingram add a second, the social construction of groups. By "social construction," they mean the recognition (by some unspecified subject) that certain shared characteristics define a discrete social group and the attribution to these shared characteristics (again by an unspecified subject) of positive or negative connotations (p. 335). This definition gives rise to a four-cell classification scheme for social groups along two dimensions: power and social construction (Figure 1). Powerful, positively constructed groups are *advantaged*; powerful, negatively constructed groups are *contenders*; weak but positively constructed groups are called *dependents*; and the unfortunate weak and negatively constructed groups are labeled *deviants*.

This classification then explains how policies allocate burdens and benefits among target populations. Advantaged groups, for example, will receive high benefits, low burdens, and control over agendas and policymaking. Deviants, not surprisingly, get low benefits, high burdens, and little control. Assuming that group power and social construction can be safely treated as independent dimensions, this scheme seems reasonable enough. It gives rise to plausible and potentially testable hypotheses about policy outcomes. At the end of this section, the authors offer the merest hint that their model, by taking into account socially constructed differences among groups, outperforms similar classifications of policy that seek to explain why different kinds of policy produce different styles of policymaking (Lowi 1964, 1972; Wilson 1973, 1980, 1992).

In the second part of the article, however, Schneider and Ingram suggest that the social construction of group identities often depends on policy and on political choices made at an earlier time (see also Ingram and Schneider 1993). In fact, they acknowledge precisely this possibility by citing Lowi's (1964) famous formulation that "policy creates politics." Policies, they conclude, affect "target populations' perceptions of democracy, inclination toward participation, and willingness to comply with policy directives," all of which undoubtedly contribute to the "social construction" of groups in a political context (p. 346). But this possibility appears to undermine the central causal claim of the first part of the article. If group social construction is at least partly a product of policy outcomes and policy outcomes are at least partly a product of group social constructions, then we are left with something of an analytical muddle.

INSTITUTIONS, HISTORY, AND SOCIAL CONSTRUCTION

The muddle arises from a formulation of "social construction" that is neither institutional nor historical. Neither institutions nor history figures in Schneider and Ingram's account of the social construction of group identity; both are essential to sorting out the causal links among the elements in their argument. Institutions are formal rules or informal patterns that organize political processes and behavior by regulating incentives and opportunities (March and Olsen 1989; North 1990; Orren and Skowronek 1993; Skocpol 1985). The configuration of a society's political institutions influences political outcomes not only by regulating formal and informal access to power but also by shaping the formation and expression of political sentiments. Over time, institutional change dramatically affects political possibilities, opening up certain roads and closing others. The ways in which social groups are defined, perceived, and made politically relevant at particular moments of history—in short, the mechanisms and meaning of social construction—are an integral part of this historical-institutional process.

Two specific shortcomings in Schneider and Ingram's formulation of "social construction" hinder their analysis. First, they take the "social construction" of a group to be a unitary phenomenon. A social group is either positively or negatively constructed—deserving or undeserving—all together and all at once. But the construction of groups in a plural society is always and everywhere a matter of contention and conflict. Some groups are viewed differently by different segments of society. Often, groups cannot even agree among themselves on a definition of their essential characteristics. Contestation over the construction of group identity is particularly acute for the most disadvantaged groups in society, groups such as African-Americans, Latinos, and women in the United States that are frequently at the center of intense policy debates. Much important politics consists precisely in such clashes over the construction of political and social group identities and perceptions (Norton 1988; Ryan et al. 1992). The shifting status and multiple nature of group identities and constructions poses a serious obstacle to a causal analysis that takes a unitary social construction as a primary explanatory variable.

Schneider and Ingram recognize this problem and wave it away at once: "Social constructions are often conflicting and subject to contention" but ". . . the actual social constructions of target groups, as well as how widely shared the constructions are, are matters for empirical analysis. Social constructions of target populations are measurable, empirical phenomena" (p. 335). Thus, they imply, we can uncover and verify unique and meaningful social constructions for groups, although they do not explain in any detail how a researcher might go about such a task. Social construction seems to mean some compilation of public attitudes toward a group as revealed in public discourse and survey data, somehow rooted in something called "objective reality" (ibid.). Moreover, among the potential sources of data about social construction that Schneider and Ingram cite (fleetingly) are statutes and legislative histories, which are themselves part of the vague dependent variable we call "policy outcomes." Here their argument verges on the tautological.

Identifying a group's "social construction" in this way is a little like identifying "public opinion" as if there were a mythical, unitary "public" with a single opinion on everything. In opinion and voting research, this problem is often modeled by referring to the median voter, who stands in the middle of a one-dimensional distribution and whose position wields enormous influence over outcomes. Are we similarly to imagine a "median constructor," whose view of a target population is controlling? Can we simply take a survey, discover that 51% of the American public thinks that university professors are lazy overpaid leeches, conclude that professors are "negatively constructed," and predict that they will not be treated so well by policymakers as a result? In a plural society there are many ideas about the social meaning of group identities. Which ideas—whose ideas—about a group are likely to find their way into policy debates and be translated into policy?

Without an account of how political institutions mediate between group identities and political outcomes, we cannot even begin to search for a satisfactory politically relevant definition of "social construction." In a given political system not all opinions or group identities are created equal. The matrix of a society's institutions and past policies influences how social groups are defined and how they relate to one another and to the state. "Social constructions" of target populations are deeply embedded in institutional configurations. Often the same group is treated differently by different policies at the same time, suggesting that target populations can be constructed differently in different institutional circumstances.

The second shortcoming of Schneider and Ingram's formulation of "social construction" is their lack of attention to history, that is, to the possibility of change over time. Group identities and social constructions do change, often quite rapidly. Some groups have been viewed quite differently at different points in history. Even assuming that the illustrative group constructions they classify according to their two-dimensional scale are "correct" and empirically verifiable, they are time-bound. "Big unions," for example, which they classify as contenders (strong, negatively constructed), have clearly not always been so. Labor unions were both stronger (in terms of membership and political influence) and more popular among the general public in the 1950s and 1960s than they are today. Both their strength and their image in American politics are products of a long history of policy decisions, institutional change, and chance (Goldfield 1987). Similarly, Schneider and Ingram classify "minorities" as contenders, although placing minorities at the high end of a power scale seems questionable. But it is easy to recall a time when African-Americans, to consider one minority, were arguably dependents (weak, positively constructed), when they were denied basic rights but forged a social and political movement that won the sympathy and respect of the nation and the world.

If the construction of group identities is embedded in institutional configurations, then institutional change has the potential to alter social constructions. As the rules and norms governing social and political interaction change, groups will face different incentives and opportunities. Existing groups will see their relations both with other groups and with the state changing; some may be better off, some worse off. New groups will mobilize to take advantage of new institutional opportunities. As these changes occur, perceptions of group behavior and social standing begin to shift, and the "social constructions" of group identities change. Institutional change, moreover, tends to lock social and political developments onto a particular path as new social arrangements become settled and it becomes increasingly costly to jump to another path (David 1985; North 1990). History matters for group identities; social constructions reflect paths of institutional development, not simply a cultural scorecard of heroes and villains.

The dynamics of institutional change and group identity can have a variety of effects that either reinforce or alter the positions of groups in the political system. In some cases, a new policy may differentiate between members of a previously defined target group, treating some members differently from others. Some part of a generally scorned group, for example, might reap benefits, perhaps unintended, from a change in policy, leaving the rest of their group behind. Over time, these new beneficiaries might come to be perceived as worthy recipients of government favor despite membership in a negatively constructed group. They might find it in their interest to mobilize to protect their gains, and their new participation might spill over into other policy arenas as well. Ultimately, policy change can lead to the redefinition and reconstruction of group identities, not through the "subtle messages of policy design" (Ingram and Schneider 1993) but through the concrete consequences of historical-institutional change. In the most optimistic scenario along these lines, public policy might be a vehicle for bringing disaffected groups into full participation and "reconnected citizenship" (Valelly 1993).

At the same time, however, policy or institutional change can reinforce the unfortunate position of the rest of the scorned group, sending them down an opposite path toward rejection, frustration, demobilization, and disengagement. Alternatively, the association of a popular policy with an unpopular group can undermine the policy rather than promoting the group's fortunes. In such cases, the developmental perspective suggests that the groups that are likely to be treated badly by public policy are precisely those groups that are powerless and scorned *because they have been treated badly by public policy in the past*. The experience of African-Americans in the American welfare state is an example of just such a dual dynamic, in which institutional change has altered the structural position of a group in the political system.

The point is not that social constructions are unimportant in policymaking but that social constructions are themselves the product of policy and institutional change. Without an account of how history and institutions affect social construction, Schneider and Ingram's scheme cannot hope to explain how social constructions change or why one or another of the outcomes just sketched might occur in any given situation. Their failure clearly to separate cause and effect in their analytical framework seriously compromises their ability to make causal inferences about the role of social construction in policy and political change. In technical terms, their framework suffers from endogeneity: their explanatory variable, social construction, is partly a consequence of their dependent variable, public policy. Endogeneity produces biased estimates of causal effects (King, Keohane, and Verba 1994, 185–96).

In fact, the very idea of "social construction" belies the possibility of fixed, static group identities. Schneider and Ingram define it as "the cultural characterizations or popular images of the persons or groups whose behavior and well-being are affected by public policy" (p. 334). The idea that meanings are constructed by popular attitudes or cultural norms inherently contradicts the proposition that things (whether words, ideas, or social categories) mean anything by themselves or have meanings that we can observe scientifically or agree on with any certainty. "Social construction" has become a fashionable and often ideologically loaded phrase throughout the academy, one that has very little meaning when applied directly and uncritically in a political context. In order to turn it into a fruitful approach to political questions, political scientists will have to specify much more carefully than have Schneider and Ingram what we mean by "social construction" and how contention over social meaning and the politics of identity might shape our politics and affect public policy. It is by now a commonplace in postpluralist political science that group identities are not fixed or "natural" in any sense. We cannot, for instance, take collective action for granted even among a category of people who manifestly have interests in common, but neither can we rule it out entirely (Hardin 1982; Olson 1965). In order to explain the role and behavior of groups in politics we must surely give some account of how group identities are constructed and consider the social and political settings in which groups are formed.

In literary criticism, meaning can be constructed and reconstructed each time we read a text. But in politics, meanings are made concrete when we pass a law or adopt a policy. At that moment, state action can institutionalize certain constructions and inhibit others, sending a social group down a particular historical path. It is undoubtedly true that socially constructed group identities play a role in shaping public policy at such moments. But what political mechanism translates social constructions into policy outcomes? What characteristics of groups and perceptions of groups are liable to change? Most important, how does social construction in a politically relevant sense occur, and who does it?

Schneider and Ingram beg this last question entirely, and their use of the passive voice in discussing social construction is emblematic of this problem: "Social constructions *are manipulated and used*. . . . When powerful, positively viewed groups *become construed negatively*, the dynamics of policy change dramatically. Some of the previously disadvantaged groups *are displaced* into a negatively constructed group that will not be able to garner as much beneficial policy. . . . When common behaviors of large numbers of ordinary people *become subject* to negative stereotyping and punishment is threatened, the expected acquiescence is unlikely" (pp. 342–43; emphasis mine). Astonishingly, the passage from which I draw the last quotation attributes civil unrest in American politics in the 1930s and 1960s to such instances of "negative stereotyping." Have Schneider and Ingram not heard of the Great Depression, the Civil Rights movement, or the Vietnam War, events that may have played some small role in producing unrest in those decades? In this case, their assumption of the importance of social construction simply obscures the most obvious historical facts. At the very least, their perspective does not help in adjudicating among competing explanations for important events in American political history.

POLITICAL CONSTRUCTION AND POLITICAL CHANGE

A more reasonable approach to the construction of group identities would consider what I will call "political construction." Rather than treating social constructions of group identity as though they came from outside the political system, a political-construction approach asks not only how group identities arise in a political setting but also how and why they become politically relevant at particular historical junctures. This approach implies, more realistically than the other, that the situation of groups with respect to society and politics depends on previous political arrangements and decisions, that is, on historically and institutionally grounded developments.

Recent attention to "policy feedbacks" in political science has called attention to this element of political change (Pierson 1992, 1993; Skocpol 1992; Weir and Skocpol 1985). Policy decisions influence groups' orientations toward and expectations of government, their incentives to organize, and their ability to present future demands or defend their gains. These changes can materially alter the "construction" of groups in social, political, and economic terms. This could conceivably happen deliberately, as Schneider and Ingram suggest: "Social constructions are manipulated and used by public officials, the media, and the groups themselves. New target groups are created, and images are developed for them; old groups are reconfigured or new images created" (p. 342). More likely, these are unintended consequences of policies and their implementation.

The characteristics of target populations can make a tremendous difference for public policy outcomes, as Schneider and Ingram argue. But understanding how and why requires attention to the political construction of group identities. The treatment of African-Americans under the Social Security Act of 1935 provides a case in point (Lieberman n.d.). The Social Security Act created welfare state institutions that were shaped in part by the racial composition of the act's target population. These institutional structures then influenced the changing construction of African-Americans in the political system. Among the policies adopted in 1935 were Old-Age Insurance (OAI, which we now call generically Social Security) and Aid to Dependent Children (ADC, now Aid to Families with Dependent Children, which we call generically "welfare"). Concentrated in the South, where they were politically powerless, economically oppressed, legally segregated, and socially scorned, African-Americans were generally excluded from the target populations for benefits under the Social Security Act. But because of institutional and political differences among the act's various policies, the consequences of exclusion both for African-Americans and for the policies themselves have been quite varied.

The target population for OAI benefits excluded over half the African-American workers in the country by excluding agricultural and domestic workers from coverage. As a result of these race-laden exclusions, however, OAI was established as (and remains) a centralized, national program under which the federal government pays benefits directly to citizens. Although it was racially exclusionary on its face, the administrative institutions of OAI incorporated African-Americans fairly from the beginning (Lieberman 1993a). The popularity of OAI and the public perception that benefits were "earned" generated political support for the program's expansion. Within 20 years, most excluded workers were brought into the OAI system, overcoming the negative racial construction that was built into the program at its creation. It has ironically become perhaps the closest thing to a race-blind social program that the United States has known. As long as African-Americans participate in the labor force, they are on the same footing as other recipients and are not deemed any less worthy than whites; yet OAI remains the most popular part of the American welfare state.

Aid to Dependent Children, on the other hand, had the potential to benefit large numbers of African-Americans, who were more likely to be poor and live in single-parent families. Because poor African-Americans were to be largely relegated to ADC, it was created with a parochial institutional structure that gave state and local officials most of the power to set eligibility rules and benefit levels, leaving the program open to racial discrimination and political manipulation. As African-Americans became more prominent on the ADC rolls, especially in northern cities, where they migrated in large numbers after World War II, the negative construction of African-Americans as undeserving welfare recipients became ever more pervasive. Rather than generating political support to overcome racial bias, ADC has been increasingly racially divisive and increasingly unpopular because of its association with race (Lieberman 1993a).

The fortunes of African-Americans in the American welfare system suggest that a simple story of "social construction" is inadequate to explain the changing relationship between racial groups and the state. The "social construction" of African-Americans in American politics is part of a complex set of structural relationships among groups (legal, political, and economic, as well as social) framed against a background of political institutions and public policies (Lieberman 1993b). The position of African-Americans in state, society, and economy in the 1930s—concrete factors such as voting restrictions, labor-repressive agriculture, and violent repression—both made it possible and provided incentives for white politicians to structure social policies in ways that constructed black citizens as unworthy beneficiaries of federal social policy. But in the 1950s, the place of African-Americans in these two social policies took dramatically opposing turns. Just when OAI accepted African-Americans as full beneficiaries, ADC increasingly began to impose burdens on black beneficiaries through a series of crackdowns and retrenchment that still continues. Clearly, a singular change in the "social construction" of African-Americans cannot account for both of these policy changes. Rather, the institutional structure of the policies themselves changed the *political* construction of African-Americans in particular ways, lifting OAI to the political stratosphere and dragging ADC into the mire of racial politics.

The changing place of African-Americans in the American welfare state suggests that a more dynamic, political-construction approach will better explain the relationship between target populations and public policy than Schneider and Ingram's static, social-construction approach. Public policies do not merely reflect the character of their target populations; policies also transform target groups by transforming the political, social, economic, and legal settings in which groups exist. The constant redefinition of group identities in the political arena is an effect of politics and policy as much as a cause. In an era of increasing racial and ethnic polarization, it is imperative that we, as political scientists, adopt a perspective and pursue a research agenda that can help us understand how political institutions both reflect and shape identities so that, as a society, we can pursue both pluralism and justice.

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RESPONSE

Misreading and misinterpretation underlie many of the criticisms Robert Lieberman levies against our June 1993 article. Most importantly, we believe he attributes to our work a purpose different from the one we were pursuing. We are unsettled that our critic paraphrases one of the central ideas in our work claiming it as an independent insight without acknowledging it as part of our argument. Robert Lieberman comes at our subject from a very different perspective, however, and one that gives far less emphasis to public policy as a factor that influences attitudes and perceptions of target groups. This difference is worth discussing, especially in light of the fact that his example of welfare policy amply illustrates the importance of public policy and the applicability of our work.

We will dispense with the lesser issues first by providing a few examples of misreading. Next, we will reiterate the central intent of our work. We then turn to consider three areas of real difference between our work and the approach advocated by Lieberman. Finally, we will describe how the case study he uses makes our case for us.

MISREADING AND RESTATEMENT

Lieberman interprets our fourfold classification scheme of *advantaged, dependents, contenders,* and *deviants* as a unitary, all-or-nothing formulation. Social constructions of target populations differ along many dimensions, varying from positive to negative, from strongly constructed to barely constructed, from heterogeneous to homogenous, from contended to virtually consensual, from rapidly changing to long stable. Having made clear the fundamental differences among quadrants that undergird our typology, we described the theoretically interesting variations in social constructions of target groups (p. 331). Far from lacking a sense of history, we wrote that the social construction of some target groups varies over time and that during the course of evolution, opinion is often divided as to the target's characteristics. To illustrate change, we provided historical examples of the evolution of social constructions (see also Ingram and Smith 1993). We hope that further research will allow political scientists better to understand the causes and patterns of consensus or division about social constructions.

We give more than the "merest hint" that our typology outperforms others (e.g., Lowi's, Wilson's). We build upon and contribute to the strong literature within public policy research relating characteristics of policy to politics. An extended discussion specifically compared our argument with Lowi's and Wilson's (pp. 344–45). We suggest that an understanding of the social construction of target populations is essential in appreciating why some people mobilize when confronted with policies that are disadvantageous to them and others do not.

Lieberman writes that we lack a clear causal argument and that what we suggest appears tautological. The difficulty, we think, is that he has condensed our logic and that the phenomenon he believes we were trying to explain is actually peripheral to our main concern. We were trying neither to trace the roots of group identity nor to explain policy outcomes. Our interest is in policy *design* and the ways public policy serves or fails to serve democracy (Ingram and Schneider 1991a, 1991b, 1993; Schneider and Ingram, 1988, 1990a, 1990b). In the article in question, policy design is an intermediate factor: our ultimate interest is citizen perceptions, orientations toward government, and political participation. The article contains dozens of causal propositions, some of which already are being tested by policy scholars (Blumenthal 1994; Brown 1995; Donovan 1994; Furlong 1994; Holmberg 1992; McDonnell 1994; White 1994).

In our theory of causation, motivations of elected officials are linked to the types of policy designs they construct, which affect people's experiences with the policy and the lessons and messages they take from it. These, in turn, influence people's values and attitudes (including their group identities), their orientations toward government, and their political participation patterns. This "final" effect becomes part of the subsequent environment that is assessed by officials as they search for policies that offer political rewards and attempt to avoid politically risky policies.

For any problem that might be considered, officials could construct a variety of policy designs. The possibilities include which persons and groups will be chosen as the target populations. Public officials have options such as whether some target groups will receive more benefits or burdens than others or whether all will be treated generally alike by the policy. There are choices among the possible rules, tools, implementation structures, and rationales.

We contend that policies are often formulated within a highly politicized environment in which officials are motivated by concern over reelection, albeit constrained by the need to appear interested in solving important societal problems. Thus officials anticipate the reactions of their challengers, the media, the target groups themselves, the opposing political party, and the broader public to their choice of target groups and to the other design elements. The dynamics of the U.S. political system motivate officials, especially elected ones, to provide beneficial policy to target populations who have political power and are constructed as *deserving* and punishment or other costs to those who lack political clout and are constructed as *undeserving*, deviant, or violent.

We hypothesize that differences in the power and social constructions of target populations will be associated with differences in each of the other elements of design. For example, advantaged populations will disproportionately receive beneficial policy delivered with capacity-building or other noncoercive tools and will have rationales that link their interests to important, broad-based, public concerns. Deviant populations will disproportionately receive punishments along with rationales that legitimate the policy.

Differences in designs result in various experiences, lessons, and messages for the target populations. Differences in experiences, messages, and lessons are associated with differences in citizen attitudes, orientations toward government, and participation patterns. We suggest that policy designs for advantaged populations send messages that their problems are important to the national interest and that they can expect to be respected (Schneider and Ingram 1993, table 1). We propose that their orientations toward government will be supportive and that they will believe that their interests coincide with the "public interest" and that others do not have such legitimate claims. The political game will seem open, fair, and winnable—because they usually win. Deviants, by contrast, receive messages that they are bad people, that their problems are their own fault, and that they can expect disrespect or even hate; government is the enemy and should be avoided. Contenders and dependents experience policies that teach lessons and send messages that are distinctly different from advantaged and deviant and that also differ from each other.

SUBSTANTIVE DIFFERENCES

Even if Robert Lieberman had followed our argument correctly, we believe he would continue to differ with us in three important areas: (1) we were writing about the social construction of *target groups*, while he is interested in the origins of *group identities;* (2) we are interested in the intricacies and effects of public policies upon democracy, while he focuses upon historical evolution of institutions and their effects on group identities; and (3) the standards we apply to evaluate evolution and change differ from his. We will consider each of these controversies in turn.

It is important to distinguish between target populations and social groups. We very carefully show that target populations are created by policy and may or may not be contiguous with any identifiable social group. In fact, we argue that public policy better serves democracy if it cuts across traditional cleavages rather than perpetuating or exacerbating existing patterns of social advantages or disadvantages. The power of policy is partly exercised through setting target boundaries that include, exclude, or divide social groups. When a target group is delineated along the same boundaries as social groups with a cohesive identity and a clear image or social construction, then the social construction of the target group will be the same as that of the social group. Public policies often cluster similarly constructed people together (separating them from those who have different constructions) and devise specific design characteristics for each. As Donovan (1994) shows in his study of AIDS, the Ryan White Act systematically singled out the more "deserving" groups of persons who have HIV/AIDS (e.g., mothers and children) for disproportionately more funding and other groups that are more negatively constructed (e.g., drug users and homosexual men) for disproportionately less funding.

Public policies can create distinctive clusters that did not previously exist. The history of juvenile justice policy is that public officials have carved from the larger population of "criminals" a separate and less reprehensible group designated as "juvenile delinquents" and from this group an even more sympathetically constructed target called "status offenders," who can be portrayed as neglected and deprived rather than deviant (Ingram and Schneider 1993, 81). The systematic differentiation of targets permits elected officials to direct punishment at some and beneficial policy toward others. Through these kinds of differentiations, public policy can reinforce prevailing constructions or can work against the grain to serve as a source of change in constructions.

We are interested in the impact of social constructions as they affect and are affected by policy, but we never suggested that policy was *the* source of social constructions and sole creator of group identities. We say more about origins of social constructions and the way they change than our critic suggests, however. Early in the article we wrote that social constructions are created by politics, media, literature, culture, socialization, history, religion and the like (p. 335). Later we refer to carriers of social constructions, specifying media, movies, literature, and music. We say that the creative imaginations and critical skills of artists, writers, journalists, academics, and others have an important impact and that these, along with dramatic events, may serve as catalysts for changing social constructions.

Lieberman contends that we neglected institutions and history. Since we focused centrally on public policy and considered the dynamics of the process, as well as historically oriented examples, we can only assume that he does not recognize public policy as a type of institution or its history as a type of history. Substituting institutions and history as intermediate variables is not an analytical improvement. The possibility that political science can illuminate the types of policy choices that will contribute to more democratic forms of politics becomes remote if vague notions of history and institutions, rather than concrete empirical concepts such as public policy, are the focus of analysis.

Institutions are given the following definition by Lieberman, which we believe is too broad in some ways and too narrow in others. He defines institutions as "formal rules or informal patterns that organize political processes and behavior by regulating incentives and opportunities." This definition is not helpful because it joins many unlike things including political parties, agencies, legislative bodies, courts, markets, families, the economic system, and (not noted by our critic) public policies.

Regulating incentives and opportunities is too narrow a view of the role of institutions in society. Lieberman's definition is similar to that used by scholars who assume people are motivated largely by rational, self-interested, instrumental behavior—a contention that we strongly reject. Drawing attention to the values, symbols, stereotypes, and images that drive behavior in nonutilitarian directions is one of the contributions of our article. Public policies are a type of institution that provides not only incentives and disincentives but that also distributes symbols and values.

Public policy is one of the institutions viewed as a primary problem solver for society. Hence, its study deserves a special place within political science rather than one lumped together with all other societal phenomena that might be called "institutions." An important contribution our article makes to political science is to focus on policy design as a subject for analysis and to provide a fuller and more textured meaning for policy than can be captured through the simplified characterizations to which public policy is sometimes reduced. Public policy is an active agent for social change and should be of far more interest to political science than broader and more deterministic notions of institutions and history. Public policy is an especially important phenomenon to study because it reflects human agency in a way that many other kinds of institutions do not. Through public policy, collective choices are made with significant consequences to how and whether problems are resolved, how benefits and costs are distributed, how target groups are viewed by themselves and others, and how such groups regard—and participate in—politics. Public policy is a complex combination of elements, including goals and objectives, agents and implementation structures, targets, tools, rules, and rationales. Each of these elements can vary in theoretically interesting ways. Together they form patterns with profound effects upon society. Charting which effects can be expected from which policy choices is relatively unmapped territory that political scientists are well positioned to explore.

The third area of substantive contention between Lieberman and ourselves relates to our treatment of evolution and change. Change is an important part of our analysis that we are evaluating along specific dimensions important to democracy and citizenship. The theory of change we consider begins with the question whether a political process dominated by the pursuit of self-interest, unequal political power among various groups in the society, and widely differentiated social constructions contains any self-correcting mechanisms such as that suggested in pluralist theories of democracy. We offer three possible paths of change.

First, we posit that the political attractiveness of providing beneficial policy to advantaged groups may result in their being the beneficiaries of so many rewards from policy that their constructions shift from "deserving" to "greedy" or selfish, with a corresponding change in the kinds of policy designs they will receive. Similarly, so many ordinary people may be swept into the highly attractive punishment policies that the negative stereotypes of "deviant" may no longer be credible, and the groups may be able to muster more effective resistance. Thus these persons may be able to increase their political power (moving toward the contender part of the continuum) or to reconstruct themselves as dependent rather than deviant, correcting the excesses of punishment-oriented policy. A second model of change is far less predictable and posits that external dramatic events, opportunities, and skillful manipulation of entrepreneurs may alter constructions. The third possibility is the path most damaging to democracy, where there *are* no self-correcting mechanisms. Instead, cycles of constructions and policies reinforce one another and continue unabated. The advantaged continue to gain in power, positive constructions, and beneficial policy even as the nation incurs increasing deficits and engages in deceptive explanations regarding who is actually receiving what from policy and why. Those constructed as deviants receive even more punishment and retribution that serves as a release for public frustration but is legitimated on the grounds that it is the only effective way to ensure domestic tranquility.

These are fine-grained propositions about possible models of policy change and in our view are a significant advance over more generic and older notions of policy feedback. In his limited discussion of policy feedback, Lieberman ignores literature that goes all the way back to David Easton's work on systems and includes the many scholars who have delved into how policy creates politics (e.g., Lowi 1972; Smith and Lipsky 1993; and contributors to Ingram and Smith 1993). How policy design both reflects contexts created in part by past policies and provides human agency for social change is a subject of continuing interest among policy scholars. The case Lieberman avers can be seen as building upon this literature and bolstering our theory.

THE WELFARE POLICY CASE

Social Security and AFDC are excellent examples of how the elements of policy design, including the delineation of targets, tools, and rationales, can have empowering and disempowering effects important to justice and citizenship. Lieberman was making our point with this case, but failed to see it. The target populations of the social security section of the original act, passed in 1935, cut across racial, ethnic, class, and economic lines and did not require means testing. The eligible groups were required to pay into the fund, thereby legitimating the policy on the grounds that all recipients were deserving and only receiving their due. That some self-employed agricultural workers and domestically employed persons were excluded in the original act does not appear to have been racially conscious, as the overwhelming proportion of such persons were white. These groups seem to have been excluded instead on the grounds that they did not fit the industrial-worker model that served as the basis of the act (Berkowitz 1991). Self-employed persons were both employee and employer, agricultural workers received significant in-kind benefits that did not count as part of their salary (e.g., food and board), and domestically employed persons worked for a large number of different people. The difficulty of keeping very good records for these employees complicated administration (ibid.).

It is not surprising that the 1950 amendments to the act brought into coverage the self-employed, agricultural workers, and domestic help, given the political attractiveness of the policy. These newly admitted groups would have to pay into the system before they could receive from it—thereby ensuring that they could be constructed as deserving and, more importantly, that they would bring enormous new resources into a system that was badly strapped for sufficient funds. One of the rationales underlying this policy change was that bringing in, for example, the domestically employed on condition of receiving benefits only if they paid into the fund would be far better than the politically unattractive alternative of including them in some kind of federally funded welfare program. Our theory suggests policy changes of exactly this type. Eligibility will be extended and benefits increased until the funds are insufficient. Policy changes to avoid bankrupting the system will occur periodically.

The targets of AFDC had from the beginning a dependent social construction. These people had very few political resources or power but were considered to be "good" people, although not particularly capable or independent. Poor and widowed mothers and their children who could not provide for themselves were the initial targets. The AFDC portion of the 1935 Social Security Act drew ideas from the mother's pension laws in the states and presupposed that mothers should be able to stay at home and care for their children. According to Berkowitz (1991), children in this group were overwhelmingly white at the time the act was passed (86%), and an estimated 85% of the recipients were children whose fathers had died.

Means tests are common features of policy for dependents and efforts to direct aid only to the truly deserving were always part of the AFDC policy design. We have argued that policy aimed toward dependents is typically undersubscribed, that there are never enough resources to serve the policy need, and that administrators and others revising policy are always pressured to establish new rules to eliminate the benefits to the least deserving (Schneider and Ingram 1994). Over time, the recipients of AFDC changed dramatically. By 1962, only 7% of the AFDC caseload involved children whose fathers had died and most were recipients because their father had left home or had never lived with them at all. The proportion of recipients who were African-American had increased substantially.

As suggested by our theory, the growing negative construction of the group has made it even more politically risky to direct beneficial policy toward them, resulting in rules and tests for eligibility that are increasingly stringent and directed to disqualify those easiest to construct negatively. Lines were proposed—and some drawn—to exclude "welfare cheats"—particularly households harboring able-bodied men, women who were able to work but did not, women who might be birthing children to increase their welfare check, women who bore a second illegitimate child, and those whose homes were ruled "unsuitable." The negative characterizations of these target groups, unfortunately, coincides with the stereotypes inflicted by racial prejudice since many welfare recipients are African-American. Current welfare reform proposals reflect social constructions of targets as deviants, badly in need of character reform to reject welfare as a way of life.

Lieberman's criticisms imply that we expect some kind of unitary change in the social construction of African-Americans to account for the inclusion of the self-employed, agricultural workers, and the domestically employed in the Social Security system (groups that are overwhelmingly white and all of whom must pay into the system if they are to receive from it) and to account for the exclusion of people, especially African-Americans, from AFDC. This reduces our theory to something quite different.

Lieberman, unwittingly it seems, actually draws our conclusion from the welfare cases, though with less attention to the way the elements of policy design deliver effect. Like us, he sees that public policies do not merely reflect the character of their target populations, they also transform target groups by transforming the political, social economic, and legal settings in which groups exist. The welfare policy case nicely illustrates one of the central lessons of our work for improving policy design for democracy. The delineation of targets in social security, which was oblivious to racial and other established cleavages and purposely included old-age survivors of all economic and social classes who had paid into the system, was the better design for democracy. It empowered older Americans and added to their political power and positive social construction. In contrast, AFDC, targeted to match economic and later racial and social classes, disempowered its intended targets. We hope our article will stimulate similar examinations of pathologies and consequences of policy designs.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN SEPTEMBER 1995

The following articles, research notes, and controversies have been tentatively scheduled for publication in a forthcoming issue.

David Austen-Smith. "Campaign Contributions and Access."

Nathaniel Beck and Jonathan N. Katz. "What To Do (and Not To Do) with Time-Series–Cross-Section Data in Comparative Politics.

Jonathan Bendor. "A Model of Muddling Through."

Sue E. S. Crawford and Elinor Ostrom. "A Grammar of Institutions."

Kathryn Firmin-Sellers. "The Politics of Property Rights."

Mark J. Gasiorowski. "Economic Crisis and Political Regime Change."

Arthur Lupia and Kaare Strom. "Coalition Termination and the Strategic Timing of Parliamentary Elections."

Tomonori Morikawa, John M. Orbell, and Audun S. Runde. "The Advantage of Being Moderately Cooperative."

Larry M. Preston. "Theorizing Difference: Voices from the Margins."

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Roy Licklider. "The Consequences of Negotiated Settlements in Civil Wars, 1945–1993." A research note.

Kevin T. McGuire and Barbara Palmer. "Issue Fluidity on the U.S. Supreme Court." A research note.

Charles J. Pattie, Ronald J. Johnston, and Edward A. Fieldhouse. "Winning the Local Vote: The Effectiveness of Constituency Campaign Spending in Great Britain, 1983–1992." A research note.

David Lowery, W. E. Lyons, and Ruth Hoogland DeHoog; Paul Teske, Mark Schneider, Michael Mintrom, and Samuel Best. "The Empirical Base of a Reinterpreted Tiebout Model." A controversy.

Cheryl Logan Sparks and Peter R. Walniuk; Theda Skocpol. "Benefits of the Ballot: Women's Suffrage Campaigns and the Enactment of Mother's Pensions." A controversy.

FUTURE SUBMISSIONS AND CORRESPONDENCE

The new Managing Editor of the *American Political Science Review*, Professor Ada W. Finifter, will be assuming her duties during the summer of 1995.

Manuscripts to be considered for publication should be sent to the current Managing Editor, Professor G. Bingham Powell, Jr., *American Political Science Review*, Department of Political Science, University of Rochester, Rochester, NY 14627-0148, if they will arrive by August 18, 1995. Manuscripts that will arrive after that date should be sent directly to Professor Ada W. Finifter, *American Political Science Review*, Department of Political Science, Michigan State University, 303 South Kedzie Hall, East Lansing, MI 48824-1032.

Correspondence concerning manuscripts *currently or previously under review* should come to the University of Rochester address until August 25, 1995. After that date all files will have been transferred and correspondence concerning them should be sent to Professor Finifter at Michigan State University.

ERRATUM

In "Civic Culture and Democracy: The Question of Causal Relationships" by Edward N. Muller and Mitchell A. Seligson, September 1994, pp. 635–653, the sentence on p. 648 should read, "He found statistical support for the inference that variation in the extent of repressive legislation against communists during the late 1940s and 1950s was a function of the level of elite political intolerance and that the level of political intolerance among the general public was not directly relevant (although it was highly correlated with the level of elite intolerance)."

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THE QUALITATIVE-QUANTITATIVE DISPUTATION: GARY KING, ROBERT O. KEOHANE, AND SIDNEY VERBA'S *DESIGNING SOCIAL INQUIRY: SCIENTIFIC INFERENCE IN QUALITATIVE RESEARCH*

Designing Social Inquiry: Scientific Inference in Qualitative Research. By Gary King, Robert O. Keohane, and Sidney Verba. Princeton: Princeton University Press, 1994. 238p. $55.00 cloth, $19.95 paper

Gary King, Robert O. Keohane, and Sidney Verba (KKV) have provoked much impassioned debate at conventions and over the information superhighway with a simple but controversial argument: the logic of good qualitative and good quantitative research is essentially the same. Their book shows how to design qualitative (small-n) studies so that they satisfy the canons of scientific inference. We asked five senior scholars, each of whose work mixes qualitative and quantitative data and methods, to evaluate the success of KKV's attempt to unify political science. David Laitin is a skeptic who wonders whether anyone can "discipline" us unruly political scientists. James Caporaso offers cautious reminders about the many varieties of qualitative research and the many meanings of falsification. David Collier examines KKV's treatment of selection bias, arguing that many of their recommendations correspond to conventional understandings that are already well-established in the field of comparative method, and that qualitative researchers sometimes have a different perspective on basic trade-offs involved in research design. Ronald Rogowski throws down the gauntlet: political scientists who have a strong theory may properly ignore some of KKV's pet "canons." Finally, Sidney Tarrow suggests that triangulating qualitative and quantitative approaches involves much more than considerations of research design. In a rejoinder, KKV reaffirm their belief that political scientists who slight design considerations ultimately hurt their own work. They conclude with a message to the discipline: good design—assuming there is good theory—produces good qualitative and quantitative political science.

DISCIPLINING POLITICAL SCIENCE

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If political science is ready to be disciplined, King, Keohane and Verba's *Designing Social Inquiry* (KKV) can do that disciplining. By this I mean that the book contains a set of concepts, rules of inference, and methodological precepts that apply to all researchers who seek a generalized and systematic understanding of politics. This does not mean that we all should be doing the same sort of research. Indeed, the rules elucidated in this book have relevance to statistically minded scholars, formal modelers, comparativists, thick describers, and interpretivists. What it does mean is that we all must remain conscious about the degree to which our own research answers an important question, so that we can accurately signal to fellow members of our discipline how much of the picture we have filled in. If we all share a common vocabulary and common standards for evaluation of evidence in light of a theory, we can become a community of scholars in common pursuit of valid knowledge. More bluntly, if we could agree upon standards of scientific inference, we could better identify our colleagues who are guilty of scientific malpractice—which, if regularly done, is a good operational indicator of a discipline. We need not, as Almond (1990) has suggested, eat at "separate tables" any longer; it is now possible productively to consume across cuisines.

Designing Social Inquiry is not itself a methodological breakthrough. Very little in it will be new or surprising to moderately well trained students in political science. What is truly innovative about this book is its catholicity. Its goal is not to exclude the "soft" side of political science from a discipline controlled by "hard-line" statisticians. Rather, its central thesis is that at root, quantitative and qualitative research in political science share a "unified logic" (p. 3). With that viewpoint, KKV's critiques of the methodological problems faced in actual qualitative research show a generosity of spirit. The book has high praise for qualitative work containing elements of good scientific practice. It also has feasible suggestions that would have improved other work that failed to meet reasonable scientific standards. Indeed, the primary goal of the book is to demonstrate to those of us on the soft side that we can approximate the standards of our brethren on the hard side if we make such an attempt. It must be noted that those veterans in quantitative work who on principle ignore "soft" research as unscientific will be disabused from their narrow viewpoint. The achievement of this book, then, is that it sets a reasonable disciplinary standard without using the young A. J. Ayer's (1946) tactic of calling all work on the soft side "metaphysics."

But disciplining has its down side, as Foucault insists in his analysis of those "discursive formations" that transcend or deconstruct disciplines (1972, 178–81). Many political scientists eagerly entered our field, perhaps from unfortunate or boring experiences in the disciplines of economics or psychology, precisely because we have been eclectic, undisciplined, and willing to tolerate a multitude of discursive formations. Brilliant young students who wish to travel to exotic places, read the classics, work out a personal utopia, or promote a political cause enter political science programs. These students may find the disciplining constraints imposed by rules of inference to be an unnecessary burden. Sensitive colleagues are willing to indulge these students, in large part because they themselves were to some extent attracted to political science because its lack of discipline was so inviting.

Students of Bakhtin would make a complementary critique. Bakhtin argued that linguistic assimilation, which is part and parcel of disciplining, leads to the emergence of "canonical" or "authoritative utterances," which themselves are capable of undermining dissent (1986, 88). A common vocabulary, from a Bakhtinian viewpoint, is never neutral. Accepting KKV's "statistical" vocabulary as bedrock could consequently lock us into a cultural framework. Indeed, their call to engage in disciplinary discourse in a language most qualitativists see as "foreign" is surely the source of anger felt by many practitioners who have read this book.

Although I am personally sympathetic with the Foucauldians and Bakhtinian's amongst us, it must be remembered that sharing a language promotes not only effective communication but also focused debate across subdisciplines. While the language of statistics does have its biases, KKV provides a conceptual apparatus that has referents in virtually all domains of our discipline. Scholars writing an article for disciplinary journals in the narrative mode, for example, will be able to use KKV's apparatus to justify its scientific merit in political science's division of labor and thereby raise its chances of appearing in these august and prestigious pages, with concomitant disciplinary rewards. Even more: scholars who strongly disagree with the statistician's bias will, after reading this book, have the tools to show its limitations to practitioners in all of political science. This book is surely the icon that iconoclasts should lust after.

It could still be objected—(along the lines I argued in Laitin 1994)—that maybe it will bring higher expected utility if statisticians learned the language of nonquantitative researchers, rather than the other way around. To this I reply that as of now, there is no contending universal vocabulary for ascertaining whether our research findings are valid. However, I would welcome a counterhegemonic project along the lines of the present one, with an alternative critical language of scientific evaluation that would be applicable in all domains of our discipline. But my welcome of alternatives in no way diminishes my admiration of the three authors of this volume for having centrally positioned their own hegemonic design.

Causes and Concepts

KKV's hegemonic project is to highlight the making of valid causal inferences as the highest goal for social inquiry. To make such inferences, researchers need to combine theory with observations in such a way as to demonstrate a causal effect. With a disciplinary division of labor, the search for valid causal inferences invites participation of scholars on both sides of our present disciplinary divide. On the one hand, the discipline is open to pure describers. Historical and anthropological interpretation are potentially fundamental for us, just so long as researchers in this mode seek to distinguish what is systematic—and what, random—in the particular events they are interpreting. Assessments of this nature will help other scholars use those studies to construct more general theory. On the other hand, the discipline must include formal modelers, if only to demonstrate through the use of mathematics the internal inconsistencies in proposed theories. But within political science, these modelers must subject their stories to systematic and unbiased tests and alter assumptions or set parameter conditions for their models when data do not confirm their theories. Historians need not make general theory; modelers need not collect systematic data; but if both are members of a common discipline, they will do their work in such a way that scholars on the other side of the divide will be able to make reasonable and productive use of their work to ensure that science advances.

The summum bonum of political science, despite KKV's admirable formulation, has never been valid causal inferences. The founders of modern social theory indeed thought otherwise. Max Weber has suggested that the essence of social theory is in the "creation of clear concepts" (Weber 1988, 278). And Emile Durkheim (1938) was especially concerned with the identification of "social facts" Indeed, concepts such as "charisma" and "the division of labor" have been longer-lasting than any valid claims about the causal effects of these concepts. It is hard to think about the political world without them, even if their causal role in any political process remains obscure. And many other such concepts guide our thinking and theorizing today, such as *cross-cutting cleavages, social mobility, prisoner's dilemma, exit/voice/loyalty, social mobilization, political culture, median voter,* and *hegemony*. Such concepts are theoretical in the sense that they combine discrete facts common to our daily life into a category, helping us to see the confusing universe in which we live in a more patterned way.

Although *Designing Social Inquiry* is at its weakest in analyzing the role of concept formation in political science, there is every reason to maintain that the development of formal criteria for such an endeavor is consistent with the authors' goals. Suppose, for example, *prisoner's dilemma* captures elements of reality in everyday life, in international relations, and in congressional committees; but we can make no useful inferences for what people will do—cooperate or defect—if they find themselves in a prisoner's dilemma. Does this mean that the concept has failed and should not be included in our theories of conflict and cooperation? Probably not. Implied by the framework provided in KKV, what we should require of researchers is that they set clear criteria to identify a prisoner's dilemma and continue to search for regularities in outcome depending on the context in which the game is played. Compelling concepts need not be part of a valid causal inference to be powerful; but to remain powerful, these concepts must be part of a research agenda that seeks to identify their systematic implications, revealing their link on a causal chain. KKV may have undervalued the crucial role of conceptual formulation in social inquiry; but this by no means is an argument to reject the disciplining that their work demands.

Critiques from within the Discipline

At a symposium devoted to *Designing Social Inquiry* held at the 1994 annual conference of the American Political Science Association, leading scholars in our discipline were far less enthusiastic than I in regard to the success of this book. Larry Bartels pointed out that the authors treated many statistical conventions (which, in reality, cover over unresolved issues) as solutions to complex epistemological problems. Reliance on these conventions, Bartells inferred, is hardly a solution to the related problems that qualitative researchers have long been addressing with their own conventions. Peter Lange argued that researchers in the area-studies tradition do not seek generality of explanation, because they hold that the "context" in which politics get played out is highly determinative of outcomes yet itself not subject to variable analysis. And Ronald Rogowski argued that some of the best work in the comparative field ignored KKV's injunctions (e.g., on never choosing cases based on codings on the dependent variable); and yet those works' high scientific status can still be justified.

The criticisms made at the APSA convention have merit. I believe two of the critiques are so fundamental as to require future revision of the text. First, KKV focus too much attention on selection criteria within a single study and undervalue the scientific practice of strategically choosing observations based upon knowledge of cases from parallel studies. If the community of scientists, rather than the individual researcher, is the unit of evaluation, some of the selection problems that King, Keohane, and Verba identify in particular studies would be partially washed away. Second, in undervaluing theory, they do not address the issue that selection criteria may be different when theory is strong as opposed to when theory is weak.

The judgment of APSA panelists was harsh indeed. But it ought to be remembered that the criticisms came from scholars who share an understanding of the bedrock concepts of our discipline that are elaborated fully in KKV. This made the criticisms powerful and interesting and allowed for focused debate. Their critiques confirmed, rather than undermined, the importance of this material for the construction of a scientific discipline.

A Plea for Utopia

This review has become something of a plea, or to use Henry Brady's label at the APSA symposium in regard to KKV, a "homily". I would hope that all of our political science curricula include the material developed in *Designing Social Inquiry*. Assigning the book in a required "logic of research" course is only one route to this goal. An alternative is to present the material in lectures, while assigning important articles and books to the students, with the goal of scrutinizing these studies to see how their authors dealt with fundamental issues of descriptive or causal inference. However presented, the concepts and precepts outlined in this book ought to become part of what Bourdiȩu (1984) would call our intellectual *habitus*. Mutual acknowledgement of work transcending the quantitative and qualitative divide should ensue. This can only spawn—and need not stifle—creativity.

And there are additional rewards for living in such a *habitus*. Suppose it became common practice at job talks, reviews for journals, and panels at disciplinary meetings to ask authors how they addressed issues of endogeneity, of multicollinearity, of possible missing variable bias, of alternative observable implications of their theory, or of their judgment concerning the number of observations necessary for valid causal inference. Such a disciplinary practice would impel all researchers to think systematically about these issues in the course of their research. They need not follow all the rules in this book. KKV recognize that in most real-world research environments, this would be impossible. But all researchers must have good scientific reasons for disregarding or modifying a particular rule. And these reasons must be made available to potential critics. The goal of making political science a discipline seems utopian, but KKV show that it is within our reach.

There is little reason, however, to be sanguine. The reaction to this book at the APSA convention gives me the impression that there is little interest in—and great opposition to—our becoming a discipline. This book will stand, then, as merely a useful exposition of statistical solutions to epistemological questions for those of us who are not statisticians. A pity that a book with such potential will play such a limited role!

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RESEARCH DESIGN, FALSIFICATION, AND THE QUALITATIVE-QUANTITATIVE DIVIDE

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While disagreement may be more interesting than agreement, I preface my remarks by saying that I am broadly sympathetic to the arguments of *Designing Social Inquiry* by King, Keohane, and Verba. The authors have tried, with considerable success, to provide unifying principles and research strategies for qualitative and quantitative research. The central argument is that the rules for descriptive and causal inference have been unnecessarily restricted to quantitative designs. Good qualitative designs also profit from variance in the explanatory variables, proper measurement strategies, and control of extraneous variation. While there are legitimate differences between qualitative and quantitative research, KKV debunk the polarized images of the systematic quantitative researcher reducing politics to rows of equations versus the qualitative scholar giving solo performances with nonreproducible insight and *Fingerspitzengefühl*. In short, the authors' "reconciliation project" provides a methodological bridge connecting qualitative and quantitative research. Some may see reconciliation as conquest, since the unity achieved does take place on a particular turf, with particular standards. Yet the results are impressive.

KKV place strategies of inquiry, or research design, at the center of the book. In teaching research methods, what I find most useful are books that encourage us to construct research strategies that bring some probing value to our questions. Some books see all problems as resolvable by sophisticated statistical manipulation. The spirit of this book is quite different. With design at the center, the central issues are choosing appropriate units, ensuring variation in explanatory variables, and controlling for confounding influences. Weak (or indeterminate) designs cannot be salvaged by clever data analysis (p. 120). Research that is structurally defective in the sense that there is no variation in explanatory variables (or more explanations than observations) is doomed to fail, no matter how insightful the analyst. Without an appropriate organizing structure, additional data and even sophisticated analysis can tell us little.

My differences with the work are framed by the fact that my methods education was heavily influenced by two people never mentioned in this book: Hubert Blalock and Donald Campbell. Blalock's sociological contributions are premised on the notion that the ecology of social science is characterized by many independent variables, all intercorrelated and imperfectly measured, with feedback effects from the dependent variable. Many methodological problems are diagnosed in this notion: overdetermination, multicollinearity, error in variables (fallible measures), and endogeneity. Each of these problems is dealt with by KKV. Thus, while Blalock's enormous contributions are not noted (perhaps because they have been assimilated into modern statistical theory), the spirit of his work is well represented in this book. On the other hand, Campbell's quasi-experimental orientation is not only omitted but rejected early in the book (p. 7). Since Campbell and Stanley's *Experimental and Quasi-experimental Designs for Research* (1963) was—and to some extent still is—an important reference for empirical researchers spanning sociology, education, psychology, political science, and policy analysis, I take up KKV's categorical rejection of this type of research.

My reactions focus on three points: (1) the nature of qualitative research, (2) the meaning of *falsificationism,* and (3) the usefulness of quasi-experimental designs. The first two points are largely agreeable elaborations of positions taken in the book. The third represents a disagreement.

The Nature of Qualitative Research

KKV strenuously argue that the same rules of inference apply to qualitative and quantitative research. While I am inclined to agree, it is because I share the authors' definition of qualitative research as research based on *in-kind* rather than *in-degrees* differences. With this distinction, variance can be of two types: across categories (e.g., types of government, gender) and across quantities of the same variable (income, degree of labor repression). In measurement theory, qualities are represented as nominal variables, and quantities, as ordinal, interval, and ratio measures. Qualitative variation is not variation in magnitude, quantitative variation is. This characterization shows that it is not really numbers that are at issue (nominal measures are assigned numbers, too) but the issue of magnitude versus quality.

With this definition of qualitative research in place, the authors easily show that a sound qualitative research strategy requires attention to the same rules of inference as a quantitative strategy ("if x, then y" is not logically different from "as x increases, y increases"). But qualitative work can be conceived differently and in ways that are more resistant to KKV's reconciliation project.

For some, qualitative research signifies something different from explanations of in-kind variation. Indeed, the whole idea of systematic research harnessed to the goal of explanation is put into question. Thick description and interpretation may serve as ends, not merely as spadework preparatory to explanation. Scholars may be interested in empathetic understanding, the interpretation of meanings, and detailed investigation of single (nonvarying) cases. Some of the book's arguments (e.g., the rules of descriptive inference) still hold. Others (e.g., the rules of causal inference) are less relevant, despite the authors' attempt to square Geertzian analysis with their project (pp. 38–41).

A related point is that KKV's arguments about differences and similarities between qualitative and quantitative research take place in a variable-centered world. This is not the only starting point. A variable-centered approach is already one in which variable properties have been abstracted from things, concrete names, and places. In the classroom, I find that the most difficult argument to make is not the unity of qualitative and quantitative research once a variable-centered model has been accepted but how one makes the transition from instances and concretely experienced sense data to variables. On this crucial issue, I know of no methodological guides. Between "Jumbo the elephant sliding down a grassy hill at Gasworks Park" and "a certain mass moving down an inclined plane with a given coefficient of friction" there is a gap.[1] Neither logic nor observation obliges us to accept the second statement once we accept the first. Yet the leap has to be taken to reach the abstract world of variables. Hitler's Reich as totalitarian regime; Austria, Norway, and Sweden as small, open, corporatist social democracies; and Brazil, Argentina, and South Korea as late-developing bureaucratic-authoritarian polities all represent examples of concept formation not forced by deductive or inductive logic (assuming one believes in the latter).

It may be that the urge to abstract is irresistible. Campbell was fond of arguing that theory and concept formation are "hard-wired in our retina," reflecting the absence of theoretical innocence in our sensory equipment. In the end, I am in agreement with the authors' but they have more careful work to do before the quantitative-qualitative gaps are bridged—and some will never be.

The Meaning of Falsificationism

Science proceeds not only by hypothesis and conjecture but also by relentless attempts to reject our own theories. This does not mean that we hope our theories are wrong but that we believe them to the extent they survive difficult tests. The falsificationist perspective is important because it emphasizes the pruning-editing-winnowing side of science (Campbell and Stanley 1963, 35) in contrast to the confirmatory perspective that attempts to assess hypotheses by discovering confirming instances.

I accept KKV's starting point—that confirmation and rejection are logically asymmetric. But the authors tend to see falsificationism in terms of deriving many implications of a theory, to increase the theory's exposure to evidence. The problem with this criterion is that there is no guarantee (or greater likelihood) that the additional derivations will be any riskier than the initial hypotheses. A developed falsificationist perspective would add three points.

First, we should consider which of our theories' implications are least likely to be confirmed if the theory is not true. This is another way of asking what the most distinctive explanatory-predictive content of the theory is. To predict that it will rain in Seattle during November is not risky.[2] Similarly, to explain why strong states win out over weaker ones (using standard definitions of capabilities) is not risky. Anomalies are those outcomes which go against the grain. They are not what our prevailing intuitions and theories would have us believe. A recent case study of bargaining outcomes illustrates the point. Lisa Martin and Kathryn Sikkink (1993) compared U.S. pressure on Argentina and Guatemala to improve their human rights records. The puzzle motivating the study was that Argentina (larger, more powerful, more autonomous) caved in to U.S. pressure, while Guatemala successfully resisted (p. 332). The authors' theory relied on a number of factors, among which was the strength of transnational human rights lobbies and organizations. Their theory is riskier in the sense that it explains an outcome different from what we would otherwise expect.

The intuition embedded in this example is that many theories are compatible with a particular outcome. Outcomes are overdetermined. In this sense, confirmation is highly equivocal in theoretical terms. Theoretically consistent outcomes are a necessary but hardly sufficient aspect of a good research strategy. Instead of finding data that correspond to theory, why not first ask which of the outcomes implied by the theory are least likely to be true if the theory is not true? This question forces us to find the "reduced set" of outcomes that are most distinctively implied by the theory. The art of good research design is to identify those cases which can tell us the most in terms of distinct theoretical content.

The second point is derivative. The authors argue that testing our theories in alternative settings is a good idea. But what guides do we have for how to conduct these tests? The falsificationist perspective provides a criterion. Elaborate the implicative core of the theory in such a way that the multiple tests reduce the set of rival hypotheses (competing explanations) as much as possible. Carrying out the same test in the same setting provides little additional support for the theory. The same test in a different setting expands the scope of a theory and may add confirmatory weight if additional factors thought to influence the outcome are taken into account. But this is hit or miss. The researcher should isolate the set of implications that has the greatest nonoverlap in competing explanations. If a theory holds across highly diverse settings, this is more impressive than confirmation under similar conditions. The presumption is that rival explanations have a greater opportunity to register their influence under diversity. This point is crucial to the most different system design (Przeworski and Teune 1970). Using Durkheim's theory of suicide as an example, Stinchcombe convincingly outlines the logic of this procedure (1968, 15–22).

Third, KKV could improve their argument by drawing out the links between falsification, quantitative reasoning, and the theoretical development of our discipline. In part, this relates to Rogowski's argument about strong theory (in the present symposium). One advantage of quantitative research is that it generates more precise predictions (often numerical values or ranges within which such values fall), which increase the difficulty of a test. Much of social science is at least implicitly about expectational standards. In statistics, one weak standard is the nondirectional null model. Findings departing from chance expectations in any direction are sufficient to reject the model. Another standard is provided by substantive theory. Do the results differ from what one expects after taking into account *x*, *y*, and *z* (this does not rule out a differently specified null model)?

My overall point is as follows. Improvements in measurement accuracy, theoretical specification, and research should yield a smaller range of allowable outcomes consistent with the predictions made. Cumulative improvements in knowledge should make our predictions riskier, more falsifiable. This seems to me to happen all too rarely in political science, in part because we are anxious to move on to new topics (skimming the cream from little investigated areas) and in part because we are more interested in presenting a "fresh look" or "new paradigm" than in using our collective achievements to define novel yet cumulative departures. We rarely report results in incremental (value-added) fashion, as additions to the existing capital stock. Instead, our results are presented as separate "findings." We are confronted with perverse incentives. To take seriously the *acquis* of social sciences has the effect of increasing the difficulty of our tests in the sense of raising the "observational hurdles" required to accept a hypothesis (Meehl 1967, 103). Conversely, to ignore past achievements makes our hypotheses easier to accept—but at great costs in terms of lowered standards and cumulation of knowledge.

Quasi-experimental Analysis

The experimental method is often considered too narrowly as a battery of techniques applicable in a laboratory but irrelevant to the "real" world. By elevating experimental *procedures* over its *logic*, we lose the opportunity to learn what experimentation implies for ex post facto research. In broad terms, the biggest achievement of experimental design is the preexperimental equivalence of groups through random assignment (Campbell and Stanley 1963, 2). The power of random assignment is often not fully appreciated in social science research. The important distinction between random assignment and random sampling is elided. Random sampling does not solve the problems of drawing inferences when numerous causal factors are associated with outcomes.[3] By contrast, the capacity of the experimenter to assign units (usually people) to treatment and control groups neutralizes nearly all subject-centered threats to validity.[4] Experimental control over the "how much" of *x* assures adequate variation in the independent variables. Control over the timing (the *when*) of exposure implies a solution to the endogeneity problem (since values of the independent variable can occur independently of the dependent variable).

The logic of experimental research provides guidelines in ex post facto settings. The random assignment technique directs us to find ways to control extraneous variables, for example, by using stratified designs that reduce variation in confounding variables or by building in variation and doing partial correlation and regression analysis. The manipulation procedure translates into the scheduling of units and observations so as to assure variation on the independent variable. Ex post facto research is the "continuation of experimental logic through other means." On this important philosophical point, I do not think there are differences with KKV. Why, then, do they reject quasi-experimental analysis?

They say "We reject the concept, or at least the word, 'quasi-experiment' " (p. 7n.). They further state that "investigator control over observations and values of the key causal variables" is the determining factor in deciding whether something is an experiment. Two points need to be made. First, researcher control over values of the independent variables is not enough to define experimentation. The ability to assign randomly is also crucial as is experimental isolation (a lab). In a pure experiment, the three properties go together. Without manipulating the independent variables, we cannot be sure that hypothesized effects will have a chance to occur. Without random assignment and laboratory isolation, we cannot be sure we would detect such effects even if they did occur.

The second point is more nuanced. If KKV mean that quasi-experimental designs do not represent a logically distinct category, I agree. However, the numerous designs pioneered by Campbell and Stanley (1963) were possible because they "unpacked" three properties that merge in pure experiments (manipulation of the independent variable, random assignment, and lab setting). These properties were then combined in various ways to produce various hybrid designs (see Achen 1986; Cook and Campbell 1979). For example, a field experiment allows for some ability to manipulate the independent variable but no control over random assignment and setting. Other designs allow random assignment (e.g., of court cases to different processing procedures) but no ability to affect the independent variables.

Quasi-experimental designs strive for three things: (1) natural settings in which abrupt variation occurs in independent variables, (2) some natural controls as one is likely to find when adjacent units of analysis experience different "treatments," and (3) a checklist of concepts and techniques to use to address internal and external validity. The interrupted time-series design and multiple control-series design provide valuable controls in many situations of interest to the political science researcher. Indeed, Campbell's (1969) "Reforms as Experiments" is a model for the policy analyst attempting to unravel complex interactions in the policy process. Because these designs are meant for a world of biased selection, differential exposure to threats to validity, measurement error, and researcher wish fulfillment, I find them helpful.

Finally, I find Campbell and Stanley's distinction between internal and external validity helpful—indeed almost necessary—to assessment of the value of any design. Internal validity is fundamental; it is the starting point: "Did *x* have an impact on *y*?" External validity asks the question, "To what other groups, units, populations can these results be extended?" The former question concerns us all, but the second is often reduced to a sampling instability issue, which it is not. It is a contextual (hence theoretical) issue: "If it is true in the Bronx, will it also hold in Cook County?," rather than "How are elements 1–30, sampled randomly from a population, different from 31–60, sampled from the same population?" The distinction is all the more helpful in that it maps nicely onto main effects versus interaction effects (main effects are threats to internal validity, interactions, to external validity). Thus, to take one example, if selection biases operate independently of one's hypothesized causal variable, it is a threat to internal validity; if these same selection factors interact with the causal variable, it is a threat to external validity. In the former case, *x* (causal variable) had no impact. In the second case, x did have an impact, but only because of its conjunction with selection factors.

Let me bring my comments back to the starting point. I regard this as a fine book that will be widely used in methodology courses. I am persuaded that much of what goes under the label of qualitative research is concerned with explanation and causality and must therefore be attentive to the main arguments of this book. By outlining a research strategy applicable in both descriptive and causal settings and relevant to qualitative and quantitative research, KKV hold the promise of unifying previously fragmented parts of our discipline. At the very least, *Designing Social Inquiry* encourages us to talk to one another and to learn more precisely where our differences lie.

Notes

I am grateful for the comments of Anthony Gill.

1. I believe this phrase, or something like it, is attributable to Sir Arthur Eddington.

2. Prediction, unlike forecast, requires a theoretical structure. The logical structure of a prediction is, "If *x* occurs, then *y* will occur." By contrast, a forecast simply asserts that *y* will occur in the future as a result of extrapolation ("casting forth") of *y*.

3. It does to some degree. Random sampling helps to eliminate chance as a factor explaining an association. However, if many variables are correlated (confounded) in the population, random sampling will only provide a more accurate assessment of this confounding. It will not control these variables in the sense of neutralizing their influence.

4. By *subject-centered* threats to validity, I mean those differences among groups which are the result of differences among the individuals that compose the groups. Random assignment does not control for differences in the environment of the groups (differences irrelevant to the treatment) or variation that the experimenter may introduce by treating the two groups differently.

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TRANSLATING QUANTITATIVE METHODS FOR QUALITATIVE RESEARCHERS: THE CASE OF SELECTION BIAS

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King, Keohane, and Verba's *Designing Social Inquiry: Scientific Inference in Qualitative Research* (KKV) is an ambitious attempt to translate for the qualitative researcher a series of insights derived from quantitative methodology. The authors build on the basic framework of regression analysis—which has been enriched over the past two decades with innovations introduced by econometricians and statisticians—to make recommendations about how qualitative analysts should confront a variety of methodological problems. KKV are strongly committed to the premise that the underlying logic of quantitative and qualitative research is basically the same (p. ix). At the same time, they are attentive to the specific dilemmas that arise in actually carrying out qualitative research, and they provide many useful examples and employ clear, nontechnical language. This is a book that moves the discussion forward, and therefore merits close attention.

Selection bias is one of the important topics into which quantitative methodologists have recently offered significant new insights. Hence, the assessment of KKV's treatment of this topic[1] provides a useful window for evaluating their effort to transpose complex issues of quantitative method to the sphere of qualitative research. This is also an interesting topic to address because KKV are centrally concerned with selection bias resulting from deliberate selection by the investigator. Their recommendations are consequently of special importance: if their diagnosis is correct, a small improvement in methodological self-awareness can yield a large improvement in scholarship. Finally, KKV's recommendations merit examination precisely because they are quite emphatic. Given their emphatic character, readers may desire assurance that they are, in fact, receiving sound advice.

The question of how to situate the problem of selection bias in relation to a spectrum of other methodological and theoretical issues is not an easy one. At one pole, in discussions of selection bias in quantitative *sociology*, one finds an influential article suggesting that the impact of selection bias is not as serious as has been believed, that efforts to introduce statistical corrections for selection bias may create more problems than they solve, and that among the many problems of quantitative analysis, this one does not merit special attention (Stolzenberg and Relles 1990). In the present context, the appropriate point of entry into the problem is different. First, in the examination of selection bias in qualitative *political research*, it is much more difficult to assess its precise impact, so that conclusions about its importance are inevitably more tentative. Second, KKV's recommendations about selection bias are centrally concerned with deliberate selection by the investigator. Hence, relevant corrections do not involve statistical procedures, but rather basic choices about case selection that are relatively easy to achieve. Third, one of KKV's central goals is to explore the interrelations among a series of different methodological problems. They are not singling out selection bias as a paramount problem: in their view, it is one of many.

I conclude that KKV offer useful recommendations regarding selection bias. Yet they subsume under this term various issues with which qualitative researchers may already be familiar, but under different labels. Obviously, in a complex field it is common to find that a given phenomenon is named in different ways. For example, what was probably the first paper ever published on selection bias referred to it as a problem of spurious correlation (Berkson 1946, 51). Nevertheless, the overlap of labels raises the question of whether KKV's methodological insights really offer something new to the qualitative analyst.

In fact, some of the important recommendations offered by KKV can just as well be viewed not as insights derived from advanced quantitative methods, but rather as part of a long-standing effort to encourage qualitative scholars to be more methodologically and theoretically aware of which cases they are analyzing. This self-consciousness can also be encouraged by insistently posing a question that, according to the traditional lore of the comparative politics subfield, should often be asked at doctoral dissertation defenses: "What is this a case of?"

Many issues that underlie this question and that are highly relevant to KKV's discussion of selection bias have previously been raised in discussions of the comparative method, that is, the branch of methodology concerned with the systematic, qualitative analysis of relatively small numbers of cases (a "small N").[2] In assessing methodological claims about selection bias, it is useful to take these earlier discussions as a base line. With regard to issues of case selection, they include the ongoing evaluation of J. S. Mill's methods of experimental inquiry, the related distinction between "most similar" and "most different" systems designs, and a new perspective on case selection in small-N studies arising from counterfactual analysis. Regarding the problem of applying concepts and indicators across diverse contexts, an issue that arises in KKV's discussion, relevant insights from work on comparative method include the traditional concern with "conceptual stretching" and the use of "system-specific," as opposed to "common," indicators. Regarding the issue of generalization, relevant insights include the argument that it may at times be appropriate for scholars to limit severely the scope of generalizations from a given set of cases, an argument with important implications for what it means to think of these cases as being selected from a larger population.

The following discussion devotes central attention to the relationship between KKV's arguments and these familiar issues in the field of comparative method. After presenting an overview of selection bias as a methodological problem, along with some initial caveats, I shall consider the potential strategy of avoiding selection bias through random sampling, examine KKV's treatment of selection bias in descriptive inference, and then explore two issues that arise in their discussion of causal inference. Finally, I shall consider how qualitative researchers understand the role of generalization in social inquiry and the implications for selection bias. Although the discussion generally supports the thesis of a convergence in the logic of quantitative and qualitative methods, it is evident that qualitative researchers at times have different priorities in designing research.

Overview of Selection Bias

Selection bias is commonly understood as occurring when the *non*random selection of cases results in inferences, based on the resulting sample, that are not statistically representative of the population. The focus of the present discussion is on selection bias deriving from deliberate selection by the investigator.[3] A common problem arising from such selection is that it may overrepresent cases at one or the other end of the distribution on a key variable. When this specifically involves the selection of cases that fall above or below a particular value on the distribution of that variable, it is referred to as a form of *truncation.*[4]

The statistical insight crucial to understanding the consequences of such selection is the observation that selecting cases so as to constrain variation toward high or low values of the *dependent* variable tends to reduce the slope estimate produced by regression analysis, whereas an equivalent mode of selection on the *explanatory* variable does not have this effect. If, for example, the analyst selects a sample that is truncated to include only cases that have higher scores on the dependent variable, the sample will tend to overrepresent cases *above* the regression line that is derived from the full data set. This mode of selection therefore gives disproportionate weight in the calculation of the slope to cases for which factors *in addition to* the principal explanatory variable play an important role in producing higher scores on the dependent variable (or lower scores, in the case of a negative relationship). As a consequence, unless the investigator can identify missing variables that explain the position of these cases above the regression line, the bivariate relationship within this subset of selected cases will appear to be *weaker* than in the larger set of cases from which they are selected. A corresponding effect occurs if selection is biased toward the lower end of the dependent variable. By contrast, if selection is biased toward the higher or lower end of the *explanatory* variable, then, as long as the underlying relationship is linear, the expected value of the slope will not change (although it may vary in particular data sets).

This asymmetry is the basis for warnings about the hazards of "selecting on the dependent variable." This expression refers not only to the deliberate selection of cases according to their scores on this variable but to any mode of selection correlated with the dependent variable (i.e., tending to select cases that have higher, or lower, values on that variable) once the effect of the explanatory variables is removed (pp. 138–39). If such a correlation exists, causal inferences will tend to be biased.

Initial Caveats

Selection bias deriving from truncation is in some respects *less* serious—and in other respects *more* serious—than might initially appear to be the case. First, as KKV note, on average it will lead analysts to underestimate the strength of causal effects, and they suggest that estimates derived from the sample may be understood as a "lower bound" in relation to the true causal effect (pp. 130, 139). In qualitative research, where the inductive character of the analysis may entail a kind of ad hoc hypothesizing that can lead to "overfitting" the data, this kind of constraint might be a useful corrective.

Second, the basic asymmetry that calls for warnings about selecting on the dependent variable applies to the slope, but *not* the correlation. The correlation is a "symmetric" measure, and constraining variance on the explanatory variable *can* affect the correlation, potentially making researchers even more vulnerable to selection bias. In quantitative research the slope is more widely used for causal inference than the correlation, in part for this reason. Yet for some specific kinds of causal analysis, the correlation (or the standardized slope, which is a closely related coefficient) is more appropriate (Achen 1982, 74–76). To the extent that it is, the warning about selection bias must be extended to the problem of selecting on the explanatory variable. It remains a topic for further investigation whether the intuitive assessment of causal relationships by qualitative researchers should be understood as more nearly analogous to the slope or the correlation—and hence whether they should be concerned with selection on the independent variable, as well as the dependent variable.

Third, advice about the distinctive problems associated with the dependent variable should be qualified in another sense as well. Selecting on the explanatory variable can affect the slope estimate under some circumstances.[5] With a bivariate linear relationship, sampling toward the high or low end of the explanatory variable does not affect the expected value of the slope. However, if the underlying relationship is nonlinear, selecting different parts of the distribution on the explanatory variable can yield different slope estimates. This is *not* due to selection bias, but it *does* involve the more general issue that case selection can influence findings. Hence, this more generic form of the problem does not arise only with selection on the dependent variable.

The Option of Random Sampling

Given that selection bias is conventionally understood as deriving from the nonrandom selection of cases, one option for the qualitative researcher might be to engage in random sampling and thereby (hopefully) avoid the whole problem. Yet KKV suggest that random sampling is not the solution.

In the statistical literature, it has been traditional to treat selection bias and sampling error that results from random selection as separate issues.[6] By contrast, KKV argue that sampling error can produce selection bias. They offer a hypothetical small-N example of three cases that have high, medium, and low scores on the dependent variable, from among which only two cases will be selected. They discuss three alternative selection rules and point out that of the three combinations of cases that can result, two will constitute a sample that is biased either toward the high or low end of the dependent variable. They conclude that "since random selection of observations is equivalent to a random choice of one of these three possible selection rules, *random selection of units in this small-n example will produce selection bias with two-thirds probability!*" (p. 126, italics added).

KKV's statement that sampling error produces selection bias with two-thirds probability entails a usage that will surprise many readers familiar with standard statistical terminology.[7] However, their goal here is to point out that with a small N and only *one* random sample, the same kind of error that is associated with selection bias is quite likely to occur. Despite the presumed virtues of random sampling, with a very small N it can produce the same kind of error that is identified with selection bias deriving from a truncated sample. Hence, they argue that the investigator is much better off engaging in a carefully planned form of nonrandom selection (pp. 125–26). This is good advice.

Descriptive Inference

KKV's discussion of selection bias in descriptive inference includes two interesting examples that will be examined here. The first concerns case studies of deterrence in international relations that focus primarily on deterrence failure (pp. 134–35). This focus has an important consequence: inferring an overall success rate of deterrence from such case studies is a big mistake. Achen and Snidal, who are cited by KKV, give the example of a prominent social scientist who used a study of 12 cases of conventional deterrence to reach the surprising conclusion that conventional deterrence fails 83.3% of the time, i.e., 10 out of 12 cases (1989, 162). Yet it does not require a deep knowledge of modern regression analysis to grasp this problem, which can readily be understood within the conventional framework of the comparative method. Thus, it is essential for researchers to keep in mind how and why the cases were originally selected, and scholars following any variant of what J. S. Mill called the "method of agreement" (1974, 388–90) should not use the cases thereby selected as a basis for descriptive generalizations concerning the dependent variable.

In another example concerned with descriptive inference, KKV consider a hypothetical assessment of support for the Liberal party in New York State, based on votes for candidates endorsed by the Liberals in elections for the State Assembly. Because the Liberals do not endorse candidates in many districts where they believe they will lose, such a study would provide an inadequate assessment of support for the party. Thus, KKV argue, descriptive inferences derived from this study would suffer from selection bias (p. 135).

Within the tradition of work on the comparative method, qualitative researchers might more readily understand this example as raising issues linked to the problem of conceptual stretching and also as a complex measurement problem that may call for system-specific indicators. Central issues here are the definition of a political party and the problem of measuring party support. If one accepts a Sartori-type definition, according to which parties are political groups that present candidates in elections to public office (1976, 64), then the Liberals are not acting as a party in those districts where they do not present candidates. This doubtless construes the definition too narrowly, but it is appropriate to emphasize that in multiparty, as opposed to two-party, systems, the practice of not running candidates in selected districts is more common, and the question of what is and is not a party is often more complex. Some conceptual reflection would seem essential here. Second, analysts who use elections as a source of data on political support would normally devote close attention to which candidates run in a particular year, because alternative candidates for a given party will generate different profiles of support. Hence, it is *not* the case that elections usually provide a straightforward measure of party support. If elections are, nonetheless, used to measure party support, some other system-specific measure should be used in the districts where the Liberals do not run candidates.

Thus, one could argue that in the case of the Liberal party in New York, using the popular vote to measure party support will produce descriptive inferences that suffer from selection bias. Alternatively, this can be viewed as a problem of conceptualization and of developing system-specific measures, topics not covered in KKV's book but that are familiar themes of comparative method.

Mild Versus Extreme Selection Bias

An important element in KKV's discussion of selection bias in causal inference is their distinction between a "milder" form of selection bias, that results from merely constraining variation on the dependent variable, and what they refer to as an "extreme" form that results from selecting only *one value* on the dependent variable—according to them, a grave mistake (p. 130). This strategy of selecting only one value may be adopted by scholars who are analyzing an outcome of exceptional interest (e.g., deterrence failure, revolutions, or high growth rates) and who wish to focus only on this outcome, out of a belief that they will thereby achieve greater insight into the phenomenon itself and into its causes. Alternatively, they may be dealing with an outcome about which previous theories, conceptualizations, measurement procedures, and empirical studies provide limited insight. Hence, they may be convinced that a carefully contextualized analysis of one or a few cases of the outcome will be more analytically productive than a broader study that compares cases of its occurrence and nonoccurrence.

This distinction between mild and extreme selection bias conflates two distinct issues with which KKV are concerned. The first is the core issue of investigator-induced bias, involving the fact that the greater the constraint on the variance of the dependent variable, the more severe the bias in inference is likely to be. The second issue is that at the outer limit, when variance on the dependent variable disappears and the investigator focuses on only one outcome on that variable, a shift to a different kind of research design has occurred. Where there is no variance, selection bias certainly may be present in that the sample may well overrepresent cases for which factors other than the main explanatory variable play an important role in accounting for their higher scores on the dependent variable. But that outcome can more usefully be treated as a different issue from the switch in research design.

Selecting One Value on the Dependent Variable

Some of the strongest criticisms regarding selection bias have been leveled against studies that focus on a single outcome on the dependent variable, which KKV characterize as an extreme form of selection bias. In such studies, according to them, "nothing whatsoever can be learned about the causes of the dependent variable without taking into account other instances when the dependent variable takes on other values" (p. 129). They suggest that the need for variation on the dependent variable "seems so obvious that we would think it hardly needs to be mentioned" and that research designs lacking such variation "are easy to deal with: avoid them!" (pp. 129, 130).

On the one hand, given that they advance a definition of "causal effect" that requires the observation of at least two different values on the dependent variable (pp. 81–82), within their own framework their position can be seen as making sense. Yet other perspectives on this question are available. In the field of comparative method, a traditional way of thinking about this design is in terms of J. S. Mill's method of agreement, a perspective that KKV note (p. 134) but do not develop. This label is used by Mill because all cases under investigation "agree" on the dependent variable. Many authors have examined the strengths and weaknesses of this design, and a standard view, expressed by Mill himself, is that this design fails to provide a positive demonstration of causation, and rather should be viewed as a "method of elimination," which can exclude causal factors if they are consistently not present when a given outcome occurs (1974, 392). As Jervis has suggested, this design may serve to assess the necessary conditions of a given outcome, or, to put it more precisely, to eliminate some hypothesized necessary conditions (1989, 194). In this sense, KKV's assertion that this type of design makes it "impossible to evaluate any individual causal effect" (p. 134) seems incomplete: it can serve to *eliminate* some hypothesized causes, which can be a useful first step in causal analysis.

A second perspective on this design becomes relevant if analysts compare cases that are matched on the dependent variable but are extremely different from one another in other respects, in which case this can be called a "most different systems" design (Przeworski and Teune 1970). One of the merits of such a design is that the challenge of distilling a common set of explanatory factors out of this diversity can push scholars to discover new explanations that might not have emerged from the analysis of a more homogeneous set of cases (Collier 1993, 112).

A third perspective is found in Fearon's discussion of counterfactuals as a means of testing hypotheses within the framework of small-N analysis (1990, 179–80). He suggests that one can make "methodological sense" of designs with no variance on the dependent variable by recognizing that scholars can employ counterfactual analysis to introduce variance, and he goes on to present a detailed discussion of how such counterfactual analysis can be carried out.

Given these three alternative perspectives, KKV's claim that "nothing whatsoever can be learned about the causes of the dependent variable" if it does not vary within a given study would seem to be excessively limiting.

Two final observations may be made about this type of design. First, it appears that studies lacking variation on the dependent variable may be less common than scholars concerned with selection bias have sometimes implied, and studies that appear to lack it may have it after all. Michael Porter's (1990) book on industrial competitiveness, analyzed by KKV, is a case in point. The authors argue that Porter focuses on 10 nations that share a common outcome on the dependent variable of "competitive advantage," a research design that "made it impossible to evaluate any causal effect" (134). However, as Porter repeatedly points out, a central concern of his study is with explaining success and failure not at the level of nations, but rather at the level of firms and industrial sectors (e.g., pp. 28–29, 33, 69, 577, 735), of which he considers both successful and unsuccessful cases. At this level, it is incorrect to state that he lacks variance on the dependent variable. He may or may not take full advantage of the variance that is present in his study, but that is a different issue. Studies can doubtless be found in which such variance is completely lacking. Yet on closer inspection, one may at times discover some variation after all. In fact, due to a scholarly instinct for "variation seeking," analysts may have a strong tendency to find some variation on the dependent variable.

The other observation concerns the real trade-offs between these different designs. If little is known about a given outcome, then the close analysis of one or two cases of its occurrence may be more productive than a broader study, focused on positive and negative cases, in which the researcher never becomes sufficiently familiar with the phenomenon under investigation to make good choices about conceptualization and measurement, which in turn can lead to conclusions of dubious validity. On the other hand, by not utilizing the comparative perspective provided by the examination of negative cases, the researcher gives up a lot. In general, it is productive to build in a comparison of contrasting outcomes.

Samples, Populations, and the Role of Generalization

Another area in which issues of selection bias intersect with discussions of comparative method concerns the relationship among samples, populations, and the issue of generalization. Discussions of selection bias by definition presume the existence of a larger set of cases, from among which the cases under analysis have in some sense been chosen. Indeed, the claim that one is selecting cases that tend toward the high end of the dependent variable is not meaningful apart from the identification of a larger set of cases that define a range for this variable. Although in some domains of research the definition of the population is clear, in many domains, as KKV (p. 125) and others have noted, it may not be clearly specified, or its definition may be a matter of debate.

In qualitative comparative studies, a central issue in the definition of the population is a fundamental ambivalence about the process of generalization to additional cases. On the one hand, the generalization of empirical findings from an initial set of cases is a basic priority of social science research, and findings that cannot be generalized are routinely considered less important. On the other hand, over the past couple of decades a concern with sensitivity to context has been stimulated by a diverse spectrum of authors.[8] This concern has led many analysts to conclude that even important theories may sometimes apply only to limited domains. If the cases under study in fact constitute the full set of theoretically relevant cases, then an issue of selection bias in relation to a larger set of cases does not arise.[9]

Within the framework of a single piece of research, a critical issue is the appropriate balance between a legitimate process of delimiting the scope of findings and a degree of particularism that excessively limits the contribution of the study. A further issue of balance arises when other analysts become interested in the findings of a given study and wish to extend them to additional cases. On the one hand, these analysts should be alert to the limitations on the scope of claims that the original author sought to impose. On the other hand, from a different theoretical or comparative perspective these other analysts might make a different decision about the appropriate scope and seek to extend the analysis to additional cases. Hence, for them a problem of selection bias could arise that was not an issue for the original author. This kind of shift can occur in any sphere of research. However, it may have special importance in areas of qualitative comparative research in which investigators are particularly concerned about imposing constraints on the scope of their findings.

KKV's arguments about selection bias are most usefully understood as pushing qualitative researchers to think about a spectrum of selection issues. These include: (1) the core problem of selection bias that has been illuminated by advanced quantitative methods, that is, the specific impact on causal inference of certain kinds of deliberate case selection; (2) other issues already familiar to many qualitative researchers, including broader questions of case selection and their implications for various approaches to descriptive and causal analysis; and (3) additional areas in which the priorities of quantitative and qualitative researchers may sometimes be quite different—as with the issue of selecting matched versus contrasting cases and the implications for selection bias of severe restrictions on claims about scope.

These points of convergence lend support to KKV's claim that the underlying logics of quantitative and qualitative research are similar. The convergence also underscores the fact that some of their important recommendations do *not* provide qualitative researchers with new methodological insights. Finally, the divergences remind us that these two traditions sometimes make different choices about underlying trade-offs entailed in the design of research.

From the perspective of qualitative researchers, the core concern that should emerge out of these discussions can again be expressed in terms of the question, "What is this a case of?" If the debate on selection bias stimulates qualitative researchers to address this question more frequently and successfully, it will have accomplished a lot.

Notes

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1. Chapter 4 discusses the overall problem of selecting cases, or observations (as KKV call them), and one third of it is specifically concerned with selection bias.

2. Most of these issues were raised in such "classic" statements on comparative method as Bendix 1963; Lijphart 1971; Przeworski and Teune 1970; Sartori 1970; and Smelser 1976.

3. On other specific contexts in which selection bias arises, see Achen 1986; Geddes 1990, 145–48; King 1989; and Przeworski and Limongi 1992, 1993.

4. Truncation can take other forms as well; see p. 142 and Moses 1968.

5. KKV make a parallel point on p. 137.

6. See, for example, the definition of sampling error in the classic *Dictionary of Statistical Terms* prepared for the International Statistical Institute (Kendall and Buckland 1960, 255–56).

7. This formulation is stated in such a way that it appears to overlook a key theoretical idea about sampling. With truncated samples, the expected value of the estimate is biased, whereas with random samples, the expected value of the estimate is unbiased in that, if the sample is drawn a sufficient number of times, the average value of the estimates provided by the samples will be equal to the parameter one is estimating. Thus in their example it would be more helpful to say that there is a two-thirds probability that *any one sample* will contain this kind of error.

8. See Geertz 1973; Przeworski and Teune 1970; Ragin 1987; and Skocpol and Somers 1980; see also Walker and Cohen's (1985) discussion of "scope statements."

9. Moses (1968, 197) and Stolzenberg and Relles (1990, 407–08) likewise argue that problems of selection bias depend on the definition of the relevant population.

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THE ROLE OF THEORY AND ANOMALY IN SOCIAL-SCIENTIFIC INFERENCE

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Designing Social Inquiry, by King, Keohane, and Verba (KKV), deserves praise for many reasons. It attempts, seriously and without condescension, to bridge the gap between qualitative and quantitative political science. It reminds a new generation of students, in both traditions, of some main characteristics of good theory (testability, operationalizability, and "leverage," or *deductive fertility*). It clarifies, even for the profoundly mathematically challenged, some of the central strictures of statistical inference (why one cannot have more variables than cases or select on the dependent variable, or why it biases results if measurement of the independent variable is faulty). It abounds with practical wisdom on research design, case selection, and complementary methodologies. Perhaps most important, it opens a dialogue between previously isolated practitioners of these two forms of analysis and provokes worthwhile discussion.

For all of these reasons and more, the book should be, will be, and—indeed, even in its *samizdat* forms—already has been widely assigned and read. It is, quite simply, the best work of its kind now available; indeed, it is very likely the best yet to have appeared.[1] At the same time, I think, *Designing Social Inquiry* falters in its aim of evangelizing qualitative social scientists; and it does so, paradoxically, because it attends insufficiently to the importance of problemation and deductive theorizing in the scientific enterprise.

As natural scientists have long understood (see Hempel, 1966), inference proceeds most efficiently by three complementary routes: (1) making clear the essential model, or process, that one hypothesizes to be at work; (2) teasing out the deductive implications of that model, focusing particularly on the implications that seem *a priori* least plausible; and (3) rigorously testing those least plausible implications against empirical reality.[2] The Nobel physicist and polymath Richard Feynman may have put it best:[3]

> Experimenters search most diligently, and with the greatest effort, in exactly those places where it seems most likely that we can prove our theories wrong. In other words we are trying to prove ourselves wrong as quickly as possible, because only in that way can we find progress. (1965, 158)

The classical example is Einstein's Theory of Relativity, which (1) uniquely provided an overarching model that could explain both the anomalies and the enduring validities of classical Newtonian mechanics, indeed could subsume it as a special case; (2) had, among its many other implications, a quite specific, rather implausible, and previously untested one about how light reflected from the planet Mercury would be deflected by the sun's gravitation; and (3) appeared at the time to be precisely accurate in this specific and implausible implication.[4] To test, however rigorously, hypotheses that challenge no deeper theory or that themselves lack deductive implications is an inefficient route of scientific inference; while theories that are precise and deductively fertile enough can often be sustained or refuted by surprisingly unelaborate tests, including ones that involve few observations or that violate normally sacrosanct principles of selection.

KKV, I contend, emphasize the third part of scientific inquiry, the rigorous testing of hypotheses, almost to the exclusion of the first two—the elaboration of precise models and the deduction of their (ideally, many) logical implications—and thus point us to a pure, but needlessly inefficient, path of social-scientific inquiry.

I can best illustrate these points by applying their strictures to some landmark works in comparative politics, often cited as worthy of emulation. Each work, as it seems to me, would fail KKV's tests and would be dismissed as insufficiently scientific. Yet in each case, I contend, the dismissal would be incorrect: the works illustrate—indeed, epitomize—valid and efficient social-scientific inquiry; and the ways in which they do so illuminate the shortcomings in the analysis under review.

Three of the classical works that I have in mind are single-observation studies; one involves three cases but all within a single region; one selects chiefly on the independent—but also on the dependent—variable, in ways deprecated by KKV; and one selects on the dependent variable. I propose (1) to sketch each briefly; (2) to argue that the conventional wisdom is right and KKV are wrong with regard to these works' worth; and (3) to reflect on the deficiencies that these works reveal in the KKV analysis.

The single-observation studies are Arend Lijphart's 1968 study of the Netherlands, *The Politics of Accommodation*; William Sheridan Allen's single-city examination, *The Nazi Seizure of Power*; and Peter Alexis Gourevitch's critique of Immanuel Wallerstein's *Modern World-System*. Each involves disconfirmation of a prevailing theory, by what Eckstein called the strategy of the "most likely" case (1975, 119).

Lijphart rightly saw in the Netherlands a serious empirical challenge to David Truman's (1951) then widely accepted theory of "cross-cutting cleavages." Truman had argued, plausibly enough, that mutually reinforcing social cleavages (class coterminous with religious practice, or religion with language) impeded social agreement and made conflict more likely. Only where each deep cleavage was orthogonal to another (e.g., Switzerland, where many Catholics are German-speaking, many Francophones Protestant) was social peace likely to endure. About the Netherlands, however, two things were abundantly clear: (1) it had virtually no cross-cutting cleavages; and (2) it had about as stable and amicable a democracy as one could find. Lijphart's study was taken at the time (I believe correctly) as having refuted Truman's theory.[5]

In attempting to explain popular support for such totalitarian movements as fascism, many social scientists had, by the 1950s, accepted a theory whose roots went back to Montesquieu and Tocqueville but whose modern version had been shaped chiefly by Lederer (1940), Arendt (1958), and—the great synthesizer of this genre—Kornhauser (1959). Again simplifying it to the point of caricature, this theory held that societies were opened to totalitarianism's Manichean zealotries by the waning (e.g., through rapid modernization) of associational life—the disappearance of those "natural" groups that afforded meaning, balance, and a sense of efficacy. Totalitarian followers were "atomized" or "mass" individuals.

Tracing the growth of the national socialist cause in a single mid-sized German town where it had prospered earlier and better than average, however, Allen (1965) found, if anything, a superabundance of associational life: singing and shooting societies, card clubs, fraternal orders, religious associations, drinking groups, and *Stammtische* of long standing, to the point that one could hardly imagine a free evening in these protofascists' lives. Neither could he observe any waning of this associational activity before or during the Nazi expansion, nor were Nazis drawn disproportionately from the less active (if anything, the contrary).[6] Only *after* Hitler came to power, with the Nazi *Gleichschaltung* of all associations, did activity decline. Allen's results were read, (again, I think, rightly) as having strongly impugned an otherwise plausible theory.

A central assertion of Immanuel Wallerstein's *Modern World-System*, volume 1 (1974), was that the "core" states of the world economy, from the sixteenth century onward, had been likeliest to develop strong states (in order to guarantee capitalist property rights and to protect trade routes) and to pursue linguistic and cultural homogeneity (in order to lower administrative and transaction costs). Yet as Gourevitch and others quickly observed, it was, in fact, a central European state of what Wallerstein had called the "semiperiphery" (i.e., Prussia) that developed arguably the strongest state in the early modern world and that came earliest to mass education and the pursuit of linguistic homogeneity (1978, esp. 423–27). The case seriously undermined this aspect of Wallerstein's theory; but Gourevitch went on to speculate—and Charles Tilly (1990) has subsequently advanced considerable argument and evidence to show—that in fact, the correlation was the reverse: the economically most advanced early modern states were often the least powerful, and vice-versa.

Against the record amassed by these and other single-observation studies, KKV contend that in general, "the single observation is not a useful technique for testing hypotheses or theories" (p. 211), chiefly because measurement error may yield a false negative, omitted variables may yield an unpredicted result, or social-scientific theories are insufficiently precise.[7] They would have us accept that the Lijphart, Allen, and Gourevitch studies—and even more the sweeping inferences that most comparativists drew from them—were bad science; as KKV state explicitly, falsification from a single observation "'is not the way social science is or should be conducted" (p. 103).

Rudolf Heberle's (1963, 1970) justly famous exploration of Nazi support in Schleswig-Holstein is exemplary in doing what KKV call "making many observations from few" (p. 217); yet it, too, would presumably fail to meet their standard. Long before Barrington Moore, Jr. (1967), solidified the thesis, analysts had conjectured a close link between labor-repressive agriculture and susceptibility to fascism. It occurred to Heberle that the north German state of Schleswig-Holstein offered an ideal test of the thesis, containing, as it did, three distinct agricultural regions, characterized respectively by (1) plantation agriculture on the East Elbian, or "Junker," model (the Hill district); (2) prosperous family farms like those of western and southwestern Germany (the Marsh); and (3) hardscrabble, quasi-subsistence farming (the *Geest*). The asserted link to feudalism would predict the earliest and strongest Nazi support in the first of these regions; but in fact, the fascist breakthrough occurred in the *Geest*, among the marginalized subsistence farmers; the family farmers came along only considerably later, and the feudal region resisted almost to the end. This brilliantly designed little study thus seriously undermined, even before its precise formulation, what has since come to be known as the "Moore thesis" about the origins of fascism.

Like Atul Kohli's (1987) three-state study of poverty policy in India, Heberle's examination inventively exploits within-country—in Heberle's case, within-region—variation. Yet KKV dismiss precisely this aspect of Kohli's analysis, on the ground that the values of both the explanatory and the dependent variables were known in advance; "Selection, in effect, is on both the explanatory and dependent variables," so that "the design provides no information about his causal hypothesis" (KKV, 145). Of course, Heberle, by confining his attention to a single state, partially constrained himself against biased selection; but Schleswig-Holstein itself might represent only random variation, and so (they would surely say) could not be taken as refuting the hypothesized causal link between feudalism and fascism. Again, I think, their strictures, taken literally, would dismiss a brilliant study as bad (or at least inadequate) social science.

My final two examples raise the stakes considerably; for they represent, by common consent, the very best of recent work in comparative politics. Yet Peter Katzenstein's *Small States in World Markets* (1985), by KKV's lights, inadmissibly restricts variation on the independent and dependent variables;

and Robert Bates's *Markets and States in Tropical Africa* (1981) impermissibly selects on the dependent variable.

Katzenstein, contesting the conventional wisdom that only large states were independent enough to be worth studying, deliberately restricted his focus to the smaller European states and, within that set, to the smaller states that were "close to the apex of the international pyramid of success," thus "excluding Ireland, Finland, and some of the Mediterranean countries" (1985, 21). His reasons were straightforward: (1) the cases that he did study were *anomalous*, for small, price-taking countries were widely supposed to face particular challenges in an uncertain international environment; and (2) they were *forerunners*, in the sense that all countries were rapidly becoming as dependent on international markets as these small ones had long been. To examine why countries that theoretically should not succeed in fact did so (reminiscent of Lijphart's strategy) and to attempt to discern a possible path of adaptation of larger states, seemed, both to Katzenstein and to his generally enthusiastic readership, a sensible strategy. Yet KKV, at least as I read them, must hold Katzenstein guilty of two cardinal sins that largely vitiate his analysis: (1) instead of choosing his cases to guarantee some range of variation on the independent variable, he restricts his analysis to small (and therefore quite trade-dependent) states; and (2) more seriously, taking economic success or failure as his dependent variable, he looks only at instances of success.

Bates's book is an even clearer case of selection on the dependent variable. Exactly as Michael Porter's *Competitive Advantage of Nations* (1990) examines only cases of economic success and thus draws withering fire from KKV (pp. 133–34), Bates focuses almost entirely on cases of economic failure or, more precisely, on the remarkably uniform *pattern* of economic failure among the states of postindependence Africa. He nonetheless develops an account that most readers have found compelling: (1) that the failures all resulted from an economic policy that heavily taxed agricultural exports to subsidize investment in heavily protected manufactures; and (2) that this self-destructive economic policy was the inevitable result of a political constellation in which urban groups were organized and powerful, rural ones scattered and weak. While Bates supports his analysis by observing that the two African cases of relative economic success (i.e., Kenya and Côte d'Ivoire) were characterized by export-friendly policy and politically more powerful farmers, this part of his discussion is brief and clearly tangential to his main argument.

Why, despite their seemingly egregious sins,[8] are all of these works believed by most comparativists—rightly, in my judgment—to have provided convincing inferences about their topics of study? Chiefly, I submit, for two reasons, which shed much light on the problems of KKV's account: (1) all of them tested, relied on, or proposed, clear and precise *theories*; and (2) all focused on *anomalies*, either in prevailing theories or in the world—cases that contradicted received beliefs or unexpected regularities that were too pronounced to be accidental.

The theories of cross-cutting cleavages (Truman 1951), atomization (e.g., Kornhauser 1959), world-systems (Wallerstein 1974), and feudal legacy (Moore 1967) had the great advantage of being precise enough to yield implications for single, or for very few, observations: Lijphart, Sheridan Allen, Gourevitch, and Heberle, respectively, took brilliant scholarly advantage of that precision: (1) to seek out anomalous cases and, usually, (2) to conjecture intelligently about a more satisfactory general theory that could avoid such anomalies.

About small states and heavy reliance on external markets there was less a prevailing theory than a prevailing prejudice—that puniness entailed constraint, insecurity, and (barring extraordinary good luck) economic trouble. By adducing seven cases of small states that had consistently prospered, Katzenstein demonstrated that insecurity and poverty were far from inevitable; by showing that their strategies, in similar circumstances, had differed, he proved that they retained considerable freedom of policy; and by analyzing their marked similarities of historical development and present-day governance, he advanced a plausible (if in this work still conjectural)[9] theory of situational requisites for highly trade-dependent states.

The African economic devastation that Bates studied was usually "explained" by a mélange of misunderstood Marxism and economic illiteracy that stressed the "dependence" of the Third World on the First. By invoking standard, simple economics, Bates easily showed that local policy, not First World plots, must be to blame: if domestic agricultural prices were systematically suppressed, one would expect to see smuggling and rural flight; if domestic industry was protected and subsidized, cartels, uncompetitive goods, and an overvalued currency; if taxes and controls poured power and resources into the hands of bureaucrats, a bloated public sector and vicious competition for place and favor. In each African case, all of these in fact prevailed, and no amount of external "dependence" could so easily explain this particular concatenation of disasters.

Yet this left a riddle no less profound than the original one: Why should almost all governments of the region have deliberately chosen policies so inimical to aggregate welfare and to long-term growth? Just as a psychologist might become intrigued if all but one or two of the people on a certain street began suddenly to mutilate themselves, Bates pursued a "cluster analysis" (see pp. 148–49) of perverse African policies and reached his highly plausible conjecture that rural weakness produced a fatal "urban bias" (see Lipton 1976) in policy.[10]

In the works of Katzenstein and Bates, then, no less than in those cited earlier, the crucial ingredient was clear, precise, powerful ("high-leverage") theory with what Lave and March (1975) tellingly called a "sense of process," that is, intuitively plausible causal links. In both accounts, universally accepted economic theory underpinned the critique of received wisdom: if small, price-taking firms survived in uncertain markets, why not small, price-taking countries; if all of the symptoms of the African cases were consistent with systematic price distortions, what other diagnosis was possible? The core of Katzenstein's alternative account was a story about how democratic corporatism facilitated flexible adjustment to external markets; the core of Bates's, a hypothesized link between power and policy. That both arguments were so clear, plausible, and precise contributed crucially to their persuasiveness.

KKV, in contrast, frequently choose as examples hypotheses that seem obvious or that lack deductive fertility. To prove, for example, that declining communist societies were likelier to spawn mass movements of opposition the less repressive the old regime was (p. 127) neither contravenes received wisdom nor carries broader implications for other cases.

The aspects of larger theory and of "sense of process," consequently seem to be sorely absent from the KKV prescriptions for social inquiry. While the authors are right to fear our natural tendency to see patterns where none exist (p. 21), they emphasize insufficiently the centrality of patterns—indeed, of "paradigms" (Kuhn, 1962)—to efficient scientific inquiry. A powerful, deductive, internally consistent theory can be seriously undermined, at least in comparative politics, by even one wildly discordant observation (Lijphart's Netherlands). On the positive side, a powerful theory can, by explaining an otherwise mysterious empirical regularity (European small-state corporatism, African economic failure), gain provisional acceptance at least as a highly plausible conjecture worthy of further research. As most discussions of spurious correlation make clear, we gain confidence in a proposed explanation to the extent that it *both* (1) fits the data *and* (2) "makes sense" in terms of its consistency with other observations and its own deductive implications. KKV, it seems to me, emphasize the former at the expense of the latter. In consequence, their advice to area specialists focuses almost entirely on "increasing the number of observations" (chap. 6). Many comparativists, I think, would instead counsel, "Choose better theory, which can make better use of few or single observations."[11]

Valuable as KKV's strictures are, I fear that devout attention to them may paralyze, rather than stimulate, scientific inquiry in comparative politics. They write eloquently and insightfully about the trade-offs between close observation of a few cases and more cursory measurement of many (chap. 2, esp. pp. 66–68); I wish they had as perceptively discussed how better theory permits inference from fewer cases, allows restriction on the independent variable, and may even profit from judicious selection on the dependent variable.

In short, I suspect they do not mean quite as stern a message as they send; or perhaps they view the studies I have discussed here in a different and more redeeming light. They would, however, have spoken more clearly to comparativists if they had specifically addressed the major literature of the less quantitative tradition.

Notes

1. The only competition, long out of print and aimed more at the advanced undergraduate level, is probably Lave and March 1975.

2. Eckstein characterized this as the strategy of the "least likely" case 1975, 118–19. See Hempel 1966, 37–38.

3. I owe this citation to Mark Lichbach.

4. "Dear Russell: Einstein's theory is completely confirmed. The predicted displacement was 1″. 72 and the observed 1″ .75 ± .06. Yours, J.E.L." (J. E. Littlewood to Bertrand Russell, 1919, quoted in Russell 1969, 149).

5. Lijphart went on to conjecture, on the basis of the Dutch case, about the precise circumstances in which non-cross-cutting cleavages were compatible with civic peace; but that is secondary to the point I am arguing here.

6. To be sure, KKV distinguish between *cases* and *observations;* and Allen's study could be read as a single case that by examining a variety of groups and individuals, encompasses many observations. Such a reading, in my view, would fundamentally misunderstand the underlying theory, whose central independent variable is the level of association that individuals encounter. Given the theory, the town (or, at most, the class within the town) is the relevant observation; and Allen's study is therefore a single case *and* a single observation.

7. Their strictures on the first two points are so sweeping that they must, by implication, include theories and hypotheses in the physical sciences. Hence I take it that they would also reject the confirmation of the theory of relativity and other cases alluded to by Hempel (1966, 77), which rested on single observations.

8. As regards selection on the dependent variable, KKV take a particularly Draconian stand: "We can. . .learn nothing about a causal effect from a study which selects observations so that the dependent variable does not vary" (p. 147).

9. To be sure, by looking only at successful small European states, Katzenstein had to leave open the possibilities (1) that unsuccessful small states were also governed corporatively and (2) that small non-European states had discovered quite different recipes for success.

10. It is worth noting that Bates has pursued this conjecture *not* through any large-n study, but by close analysis of an apparently anomalous case: Colombia, where dispersed coffee farmers of modest means prevailed politically not only against citydwellers but over concentrated plantation owners of considerable wealth.

11. As I note at the outset, KKV do discuss—at some length and quite sensibly—some major characteristics of good theory (sec. 3.5). They seem, however to despair that social-scientific theories can ever be precise enough to permit valid inference from few cases (pp. 210–11); and they explicitly reject parsimony as an inherently desirable property of social-scientific theory (pp. 20, 104–5). On neither point, I suspect, will most comparativists find their arguments persuasive; and they seem to me to be refuted by the examples I adduce here.

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BRIDGING THE QUANTITATIVE-QUALITATIVE DIVIDE IN POLITICAL SCIENCE

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In *Designing Social Inquiry,* Gary King, Bob Keohane and Sidney Verba (KKV) have performed a real service to qualitative researchers. I, for one, will not complain if I never again have to look into the uncomprehending eyes of first-year graduate students when I enjoin them (pace Przeworski and Teune) to "turn proper names into variables." The book is brief and lucidly argued and avoids the weighty, muscle-bound pronouncements that are often studded onto the pages of methodological manuals.

But following KKV's injunction that "a slightly more complicated theory will explain vastly more of the world" (p. 105), I will praise them no more but focus on an important weakness in the book. Their central argument is that the same logic that is "explicated and formalized clearly in discussions of quantitative research methods" underlies—or should—the best qualitative research (p. 4). If this is so, then they really ought to have paid more attention to the *relations* between quantitative and qualitative approaches and what a rigorous use of the latter can offer quantifiers. But while they offer a good deal of generous (at times patronizing) advice to qualitatively oriented scholars, they say very little about how qualitative approaches can be combined with quantitative research. Especially with the growth of choice-theoretic approaches, whose users often illustrate their theories with stories, there is a need for a set of ground rules on how to make intelligent use of qualitative data.

KKV do not address this issue. Rather, they use the model of quantitative research to advise qualitative researchers on how best to approximate good models of descriptive and causal inference. (Increasing the number of observations is their cardinal operational rule.) But in today's social science world, how many social scientists can be simply labeled "qualitative" or "quantitative"? How often, for example, do we find support for sophisticated game-theoretic models resting on the use of anecdotal reports or on secondary evidence lifted from one or two qualitative sources? More and more frequently in today's social science practice, quantitative and qualitative data are interlarded within the same study. A recent work that KKV warmly praise illustrates both that their distinction between quantitative and qualitative researchers is too schematic and that we need to think more seriously about the interaction of the two kinds of data.

Marinating Putnam

In Robert Putnam's (1993) analysis of Italy's creation of a regional layer of government, *Making Democracy Work,* countless elite and mass surveys and ingenious quantitative measures of regional performance are arrayed for a 20-year period of regional development. On top of this, he conducted detailed case studies of the politics of six Italian regions, gaining, in the process, what KKV recommend as "an intimate knowledge of the internal political manoeuvering and personalities that have animated regional politics over the last two decades" (p. 5) and Putnam calls "marinating yourself in the data" (Putnam 1993, 190). KKV use *Making Democracy Work* to praise the virtues of "soaking and poking," in the best Fenno tradition (p. 38).

But Putnam's debt to qualitative approaches is much deeper and more problematic than this; for after spending two decades administering surveys to elites and citizens in the best Michigan mode, he was left with the task of explaining the sources of the vast differences he had found between Italy's north-central and southern regions. To find them, his quantitative evidence offered only indirect evidence; and he turned to history, repairing to the halls of Oxford, where he delved deep into the Italian past to fashion a provocative interpretation of the superior performance of the northern Italian regional governments vis-à-vis the southern ones. This he based on the civic traditions of the (northern) Renaissance city-states, which, according to him, provide "social capital" that is lacking in the traditions of the South (chap. 5). A turn to qualitative history (probably not even in Putnam's mind when he designed the project) was used to interpret cross-sectional, contemporary quantitative findings.

Putnam's procedure in *Making Democracy Work* pinpoints a problem in melding quantitative and qualitative approaches that KKV's canons of good scientific practice do not help to resolve. For in delving into the qualitative data of history to explain our quantitative findings, by what rules can we choose the *period* of history that is most relevant to our problem? And what *kind* of history are we to use; the traditional history of kings and communes or the history of the everyday culture of the little people? And how can the effect of a particular historical period be separated from that of the periods that precede or follow it? In the case of *Making Democracy Work,* for example, it would have been interesting to know (as Suzanne Berger asked at the 1994 APSA roundtable devoted to the book) by what rules of inference Putnam chose the Renaissance as determining of the North's late twentieth-century Italian civic superiority. Why not look to its sixteenth-century collapse faced by more robust monarchies, its nineteenth-century military conquest of the South, or its 1919–21 generation of fascism (not to mention its 1980s corruption-fed pattern of economic growth)? None of these are exactly "civic" phenomena; by what rules of evidence are they less relevant in "explaining" the northern regions' civic superiority over the South than the period of the Renaissance city-states? Putnam does not tell us; nor do KKV.

To generalize from the problem of Putnam's book, qualitative researchers have much to learn from the model of quantitative research. But their quantitative cousins who wish to profit from conjoining their findings with qualitative sources need, for the selection of qualitative data and the intersection of the two types, rules just as demanding as the rules put forward by KKV for qualitative research on its own. I shall sketch some useful approaches to bridging the quantitative-qualitative gap from recent examples of comparative and international research.

Tracing Processes To Interpret Decisions

One such rule that KKV cite favorably is the practice of *process tracing*, in which the researcher looks closely at 'the decision process by which various initial conditions are translated into outcomes' " (p. 226, quoting George and McKeown 1985, 35). But even here, KKV interpret the advantages of process tracing narrowly, assimilating it to their favorite goal of increasing the number of theoretically relevant observations (p. 227). As George and McKeown actually conceived it, the goal of process tracing was not to increase the number of discrete decision stages and aggregate them into a larger number of data points but to *connect* the phases of the policy process and enable the investigator to identify the reasons for the emergence of a particular decision through the dynamic of events (George and McKeown 1985, 34–41). Process tracing is different *in kind* from observation accumulation and is best employed in conjunction with it—as was the case, for example, in the study of cooperation on economic sanctions by Lisa Martin (1992) that KKV cite so favorably.

Systematic and Nonsystematic Variable Discrimination

KKV give us a second example of the uses of qualitative data but, once again, underestimate its particularity. They argue that the variance between different phenomena "can be conceptualized as arising from two separate elements: *systematic* and *nonsystematic* differences," the former more relevant to fashioning generalizations than the latter (p. 56). For example, in the case of conservative voting in Britain, systematic differences include such factors as the properties of the district, while unsystematic differences could include the weather or a flu epidemic at the time of the election. "Had the 1979 British elections occurred during a flu epidemic that swept through working-class houses but tended to spare the rich," they conclude, "our observations might be rather poor measures of underlying Conservative strength" (pp. 56–57).

Right they are, but this piece of folk wisdom hardly exhausts the importance of nonsystematic variables in the interpretation of quantitative data. A good example comes from how the meaning and extension of the strike changed as systems of institutionalized industrial relations developed in the nineteenth century. At its origins, the strike was spontaneous, uninstitutionalized, and often accompanied by whole-community "turnouts." As unions developed and governments recognized workers' rights, the strike broadened to whole sectors of industry, became an institutional accompaniment to industrial relations, and lost its link to community collective action. The systematic result of this change was permanently to affect the patterns of strike activity. Quantitative researchers like Michelle Perrot (1986) documented this change. But had she regarded it only as a case of "nonsystematic variance" and discarded it from her model, as KKV propose, Perrot might well have misinterpreted the changes in the form and incidence of the strike rate. Because she was as good a historian as she was a social scientist, she retained it as a crucial change that transformed the relations between the strike incidence and industrial relations.

To put this more abstractly, distinct historical events often serve as the tipping points that explain the interruptions in an interrupted time-series, permanently affecting the relations between the variables (Griffin 1992). Qualitative research that turns up "nonsystematic variables" is often the best way to uncover such tipping points. Quantitative research can then be reorganized around the shifts in variable interaction that such tipping points signal. In other words, the function of qualitative research is not only, as KKV seem to argue, to peel away layers of unsystematic fluff from the hard core of systematic variables but also to assist researchers to understand shifts in the value of the systematic variables.

Framing Qualitative Research within Quantitative Profiles

These two uses of qualitative data pertain largely to aiding quantitative research. But this is not the only way in which social scientists can combine quantitative and qualitative approaches. Another is to focus on the qualitative data, using a systematic quantitative data base as a frame within which the qualitative analysis is carried out. Case studies have been validly criticized as being based on often dramatic but frequently unrepresentative cases. Studies of successful social revolutions often possess characteristics that may also be present in unsuccessful revolutions, rebellions, riots, and ordinary cycles of protest (Tilly 1993, 12–14). In the absence of an adequate sample of revolutionary episodes, no one can ascribe particular characteristics to a particular class of collective action.

The representativity of qualitative research can never be wholly assured until the cases become so numerous that the analysis comes to resemble quantitative research (at which point the qualitative research risks losing its particular properties of depth, richness and process tracing). But framing it within a quantitative data base makes it possible to avoid generalizing on the occasional "great event" and points to less dramatic—but cumulative—historical trends.

Scholars working in the "collective action event" history tradition have used this double strategy with success. For example, in his 1993 study of over seven hundred revolutionary years in over five hundred years of European history, Charles Tilly assembled data that could have allowed him to engage in a large-N study of the correlates and causes of revolution. Tilly knows how to handle large time-series data sets as well as anybody. But he did not believe that the concept of *revolution* had the monolithic quality that other social scientists had assigned to it (1993, chap. 1). So he resisted the temptation for quantification, using his data base, instead, to frame a series of regional time-series narratives that depended as much on his knowledge of European history as on the data themselves. When a problem cried out for systematic quantitative analysis (e.g., when it came to periodizing nationalism), Tilly (1994) was happy to exploit the quantitative potential of the data. But the quantitative data set served mainly as a frame for qualitative analysis of representative regional and temporal revolutionary episodes and series of episodes.

Putting Qualitative Flesh on Quantitative Bones

These examples are possibly exotic to the traditions of much of American social science practice. But an American sociologist, Doug McAdam, has shown how social science can be enriched by combining quantitative and qualitative approaches to the same data base. McAdam's 1988 study of Mississippi Freedom Summer participants was based on a treasure-trove of quantifiable data—the original questionnaires of the prospective Freedom Summer volunteers. While some of these young people eventually stayed home, others went south to register voters, teach in "freedom schools" and risk the dangers of Ku Klux Klan violence. Two decades later, both the volunteers and the no-shows could be interviewed by a researcher with the energy and the imagination to go beyond the use of canned data banks.

McAdam's main analytic strategy was to carry out a paired comparison between the questionnaires of the participants and the stay-at-homes and to interview a sample of the former in their current lives. This systematic comparison formed the analytical spine of the study and of a series of technical papers. But except for a table or two in each chapter, the texture of *Freedom Summer* is overwhelmingly qualitative. McAdam draws on his interviews with former participants, as well as on secondary analysis of other people's work, to get inside the Freedom Summer experience and to highlight the effects that participation had on their careers and ideologies and their lives since 1964. With this combination of quantitative and qualitative approaches, he was able to tease a convincing picture of the effects of Freedom Summer activism from his data.

As I write this, I imagine KKV exclaiming, "But this is *precisely* the direction we would like to see qualitative research moving—toward expanding the number of observations and respecifying hypotheses to allow them to be tested on different units!" (see chap. 6). But would they argue, as I am, that it is the *combination* of quantitative and qualitative methods trained on the same problem (not a move toward the logic of quantitative analysis alone) that is desirable? Two more ways of combining these two logics illustrate my intent.

Sequencing Quantitative and Qualitative Research

The growth industry of qualitative case studies that followed the 1980–81 Solidarity movement in Poland largely took as given the idea that Polish intellectuals had the most important responsibility for the birth and ideology of this popular movement. There was scattered evidence for this propulsive role of the intellectuals; but since most of the books that appeared after the events were written by them or by their foreign friends, an observer bias might have been operating to inflate their importance in the movement vis-à-vis the working class that was at the heart of collective action in 1980–81 and whose voice was less articulate.

Solid quantitative evidence came to the rescue. In a sharp attack on the "intellectualist" interpretation and backed by quantitative evidence from the strike demands of the workers themselves, Roman Laba showed that their demands were overwhelmingly oriented toward trade union issues and showed little or no effect of the proselytizing that Polish intellectuals had supposedly been doing among the workers of the Baltic coast since 1970 (1991, chap. 8). This finding dovetailed with Laba's own qualitative analysis of the development of the workers' movement in the 1970s and downplayed the role of the Warsaw intellectuals who had been at the heart of a series of books by their foreign friends.

The response of those who had been responsible for the intellectualist interpretation of Solidarity was predictably violent. But there were also more measured responses that shed new light on the issue. For example, prodded by Laba's empirical evidence of worker self-socialization, Jan Kubik returned to the issue with both a sharper analytical focus and better qualitative evidence than the earlier intellectualist theorists had employed, criticizing Laba's conceptualization of class and reinterpreting the creation of Solidarity as "a multistranded and complicated social entity . . . created by the contributions of various people" whose role and importance he proceeded to demonstrate (1994, 230–38). Moral: a sequence of contributions using different kinds of evidence led to a clearer and more nuanced understanding of the role of different social formations in the world's first successful confrontation with state socialism.

Triangulation

I have left for last the research strategy that I think best embodies the strategy of combining quantitative and qualitative methods—the *triangulation* of different methods on the same problem. Triangulation is particularly appropriate in cases in which quantitative data are partial and qualitative investigation is obstructed by political conditions. For example, Valerie Bunce used both case methodology and quantitative analysis to examine the policy effects of leadership rotation in Western and socialist systems. In *Do New Leaders Make a Difference?*, she wrote, "I decided against selecting one of these approaches to the neglect of the other [the better] to test the impact of succession on public policy by employing *both* methodologies" (1981, 39).

Triangulation is also appropriate in specifying hypotheses in different ways. Consider the classical Tocquevillian insight that regimes are most susceptible to a political opportunity structure that is partially open. The hypothesis takes shape in two complementary ways: (1) that liberalizing regimes are more susceptible to opposition than either illiberal or liberal ones; and (2) that within the same constellation of political units, opposition is greatest at intermediate levels of political opportunity. Since there is no particular advantage in testing one version of the hypothesis over the other, testing both is optimal (as can be seen in the recent social movement study, Kriesi et al. 1995).

My final example of triangulation comes, with apologies, from my own research on collective action and social movements in Italy. In the course of a qualitative reconstruction of a left-wing Catholic "base community" that was active in a peripheral district of Florence in 1968, I found evidence that linked this movement discursively to the larger cycle of student and worker protest going on in Italy at the same time (Tarrow 1988). Between 1965 and 1968, its members had been politically passive, focusing mainly on neighborhood and educational issues. But as the worker and student movements exploded around it in 1968, their actions became more confrontational, organized around the themes of autonomy and internal democracy that were animating the larger worker and student movements around them.

Researchers convinced of their ability to understand political behavior by interpreting "discourse" might have been satisfied with these observations; but I was not. If nothing else, Florence was only one case among potential thousands. And in today's global society, finding thematic similarity among different movements is no proof of direct diffusion, since many movements around the world select from the same stock of images and frames without the least connection among them (Tarrow 1994, chap. 11).

As it happened, quantitative analysis came to the rescue to triangulate on the same problem. For a larger study, I had collected a large sample of national collective action events for a period that bridged the 1968 Florentine episode. And as it also happened, two Italian researchers had collected reliable data on the total number of religious "base communities" like the Florentine one throughout the country (Sciubba and Pace 1976). By reoperationalizing the hypothesis cross-sectionally, I was able to show a reasonably high positive correlation (R = .426) between the presence of Catholic base communities in various cities and the magnitude of general collective action in each city (Tarrow 1989, 200). A longitudinal, local, and qualitative case study triangulated with the results of cross-sectional, national, and quantitative correlations to turn my intuitive hunch that Italy in the 1960s underwent an integrated cycle of protest into a more strongly supported hypothesis.

KKV are not among those social scientists who believe that quantification is the answer to all the problems of social science research. But their single-minded focus on the logic of quantitative research (and of a certain *kind* of quantitative research) leaves underspecified the particular contributions that qualitative approaches make to scientific research, especially when combined with quantitative research. As quantitatively trained researchers shift to choice-theoretic models backed up by illustrative examples (often containing variables with different implicit metrics), the role of qualitative research grows more important. We are no longer at the stage when public choice theorists can get away with demonstrating a theorem with an imaginary aphorism. We need to develop rules for a more systematic use of qualitative evidence in scientific research. Merely wishing that it would behave as a slightly less crisp version of quantitative research will not solve the problem.

This is no plea for the veneration of historical uniqueness and no argument for the precedence of "interpretation" over inference. (For an excellent analysis of the first problem, see KKV pp. 42–3 and of the second, pp. 36–41.) My argument, rather, is that a single-minded adherence to *either* quantitative or qualitative approaches straightjackets scientific progress. Whenever possible, we should use qualitative data to interpret quantitative findings, to get inside the processes underlying decision outcomes, and to investigate the reasons for the tipping points in historical time-series. We should also try to use different kinds of evidence together and in sequence and look for ways of triangulating different measures on the same research problem.

KKV have given us a spirited, lucid, and well-balanced primer for training our students in the essential unity of social science work. Faced by the clouds of philosophical relativism and empirical nominalism that have recently blown onto the field of social science, we should be grateful to them. But their theoretical effort is marred by the narrowness of their empirical specification of qualitative research and by their lack of attention to the qualitative needs of quantitative social scientists. I am convinced that had a final chapter on combining quantitative and qualitative approaches been written by these authors, its spirit would not have been wildly at variance with what I have argued here. As it is, someone else will have to undertake that effort.

Notes

I wish to thank Henry Brady, Miriam Golden, Peter Katzenstein, David Laitin, Peter Lange, Doug McAdam, Walter Mebane, Robert Putnam, Shibley Telhami and Charles Tilly for their comments on drafts of this review.

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THE IMPORTANCE OF RESEARCH DESIGN IN POLITICAL SCIENCE

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Receiving five serious reviews in this symposium is gratifying and confirms our belief that research design should be a priority for our discipline. We are pleased that our five distinguished reviewers appear to agree with our unified approach to the logic of inference in the social sciences, and with our fundamental point: that good quantitative and good qualitative research designs are based fundamentally on the same logic of inference. The reviewers also raised virtually no objections to the main practical contribution of our book—our many specific procedures for avoiding bias, getting the most out of qualitative data, and making reliable inferences.

However, the reviews make clear that although our book may be the latest word on research design in political science, it is surely not the last. We are taxed for failing to include important issues in our analysis and for dealing inadequately with some of what we included. Before responding to the reviewers' most direct criticisms, let us explain what we emphasize in *Designing Social Inquiry* and how it relates to some of the points raised by the reviewers.

WHAT WE TRIED TO DO

Designing Social Inquiry grew out of our discussions while coteaching a graduate seminar on research design, reflecting on job talks in our department, and reading the professional literature in our respective subfields. Although many of the students, job candidates, and authors were highly sophisticated qualitative and quantitative data collectors, interviewers, soakers and pokers, theorists, philosophers, formal modelers, and advanced statistical analysts, many nevertheless had trouble defining a research question and designing the empirical research to answer it. The students proposed impossible fieldwork to answer unanswerable questions. Even many active scholars had difficulty with the basic questions: What do you want to find out? How are you going to find it out? and, above all, How would you know if you were right or wrong?

We found conventional statistical training to be only marginally relevant to those with qualitative data. We even found it inadequate for students with projects amenable to quantitative analysis, since social science statistics texts do not frequently focus on research *design* in observational settings. With a few important exceptions, the scholarly literatures in quantitative political methodology and other social science statistics fields treat existing data and their problems as given. As a result, these literatures largely ignore research design and, instead, focus on making valid inferences through statistical corrections to data problems. This approach has led to some dramatic progress; but it slights the advantage of improving research design to produce better data in the first place, which almost always improves inferences more than the necessarily after-the-fact statistical solutions.

This lack of focus on research design in social science statistics is as surprising as it is disappointing, since some of the most historically important works in the more general field of statistics are devoted to problems of research design (see, e.g., Fisher (1935) *The Design of Experiments*). Experiments in the social sciences are relatively uncommon, but we can still have an enormous effect on the value of our qualitative or quantitative information, even without statistical corrections, by improving the design of our research. We hope our book will help move these fields toward studying innovations in research design.

We culled much useful information from the social science statistics literatures and qualitative methods fields. But for our goal of explicating and unifying the logic of inference, both literatures had problems. Social science statistics focuses too little on research design, and its language seems arcane if not impenetrable. The numerous languages used to describe methods in qualitative research are diverse, inconsistent in jargon and methodological advice, and not always helpful to researchers. We agree with David Collier that aspects of our advice can be rephrased into some of the languages used in the qualitative methods literature or that used by quantitative researchers. We hope our unified logic and, as David Laitin puts it, our "common vocabulary" will help foster communication about these important issues among all social scientists. But we believe that any coherent language could be used to convey the same ideas.

We demonstrated that "the differences between the quantitative and qualitative traditions are only stylistic and are methodologically and substantively unimportant" (p. 4). Indeed, much of the best social science research can combine quantitative and qualitative data, precisely because there is no contradiction between the fundamental processes of inference involved in each. Sidney Tarrow asks whether we agree that "it is the *combination* of quantitative and qualitative" approaches that we desire (p. 473). We do. But to combine both types of data sources productively, researchers need to understand the fundamental logic of inference and the more specific rules and procedures that follow from an explication of this logic.

Social science, both quantitative and qualitative, seeks to develop and evaluate theories. Our concern is less with the development of theory than *theory evaluation*—how to use the hard facts of empirical reality to form scientific opinions about the theories and generalizations that are the hoped for outcome of our efforts. Our social scientist uses theory to generate *observable implications*, then systematically applies publicly known procedures to infer from evidence whether what the theory implied is correct. Some theories emerge from detailed observation, but they should be evaluated with new observations, preferably ones that had not been gathered when the theories were being formulated. Our logic of theory evaluation stresses maximizing leverage—explaining as much as possible with as little as possible. It also stresses minimizing bias. Lastly, though it cannot eliminate uncertainty, it encourages researchers to report estimates of the uncertainty of their conclusions.

Theory and empirical work, from this perspective, cannot productively exist in isolation. We believe that it should become standard practice to demand clear implications of theory and observations checking those implications derived through a method that minimizes bias. We hope that *Designing Social Inquiry* helps to "discipline political science" in this way, as David Laitin recommends; and we hope, along with James Caporaso, that "improvements in measurement accuracy, theoretical specification, and research should yield a smaller range of allowable outcomes consistent with the predictions made" (p. 459).

Our book also contains much specific advice, some of it new and some at least freshly stated. We explain how to distinguish systematic from nonsystematic components of phenomena under study and focus explicitly on trade-offs that may exist between the goals of unbiasedness and efficiency (chap. 2). We discuss causality in relation to counterfactual analysis and what Paul Holland calls the "fundamental problem of causal inference" and consider possible complications introduced by thinking about causal mechanisms and multiple causality (chap. 3). Our discussion of counterfactual reasoning is, we believe, consistent with Donald Campbell's "quasi-experimental" emphasis; and we thank James Caporaso for clarifying this.[1]

We pay special attention in chapter 4 to issues of what to observe: how to avoid confusion about what constitutes a "case" and, especially, how to avoid or limit selection bias. We show that selection on values of explanatory variables does not introduce bias but that selection on values of dependent variables does so; and we offer advice to researchers who cannot avoid selecting on dependent variables.

We go on in chapter 5 to show that while random measurement error in dependent variables does not bias causal inferences (although it does reduce efficiency), measurement error in explanatory variables biases results in predictable ways. We also develop procedures for correcting these biases even when measurement error is unavoidable. In that same chapter, we undertake a sustained analysis of endogeneity (i.e., when a designated "dependent variable" turns out to be causing what you thought was your "explanatory variable") and omitted variable bias, as well as how to control research situations so as to mitigate these problems. In the final chapter, we specify ways to increase the information in qualitative studies that can be used to evaluate theories; we show how this can be accomplished without returning to the field for additional data collection. Throughout the book, we illustrate our propositions not only with hypothetical examples but with reference to some of the best contemporary research in political science.

This statement of our purposes and fundamental arguments should put some of the reviewers' complaints about omissions into context. Our book is about doing empirical research designed to evaluate theories and learn about the world—to make inferences—not about generating theories to evaluate. We believe that researchers who understand how to evaluate a theory will generate better theories—theories that are not only more internally consistent but that also have more observable implications (are more at risk of being wrong) and are more consistent with prior evidence. If, as Laitin suggests, our singlemindedness in driving home this argument led us implicitly to downgrade the importance of such matters as concept formation and theory creation in political science, this was not our intention.

Designing Social Inquiry repeatedly emphasizes the attributes of good theory. How else to avoid omitted variable bias, choose causal effects to estimate, or derive observable implications? We did not offer much advice about what is often called the "irrational nature of discovery," and we leave it to individual researchers to decide what theories they feel are worth evaluating. We do set forth some criteria for choosing theories to evaluate—in terms of their importance to social science and to the real world—but our methodological advice about research design applies to any type of theory. We come neither to praise nor to bury rational-choice theory, nor to make an argument in favor of deductive over inductive theory. All we ask is that whatever theory is chosen be evaluated by the same standards of inference. Ronald Rogowski's favorite physicist, Richard Feynman, explains clearly how to evaluate a theory (which he refers to as a "guess"): "If it disagrees with [the empirical evidence], it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, who made the guess, or what his name is—if it disagrees with [the empirical evidence] it is wrong. That is all there is to it" (1965, 156).[2]

One last point about our goal: we want to set a high standard for research but not an impossible one. All interesting qualitative and quantitative research yields uncertain conclusions. We think that this fact ought not to be dispiriting to researchers but should rather caution us to be aware of this uncertainty, remind us to make the best use of data possible, and energize us to continue the struggle to improve our stock of valid inferences about the political world. We show that uncertain inferences are every bit as scientific as more certain ones so long as they are accompanied by honest statements of the degree of uncertainty accompaning each conclusion.

OUR ALLEGED ERRORS OF OMISSION

The major theme of what may seem to be the most serious criticism offered above is stated forcefully by Ronald Rogowski. He fears that "devout attention" to our criteria would "paralyze, rather than stimulate, scientific inquiry." One of Rogowski's arguments, echoed by Laitin, is that we are too obsessed with increasing the amount of information we can bring to bear on a theory and therefore fail to understand the value of case studies. The other major argument, made by both Rogowski and Collier is that we are too critical of the practice of selecting observations according to values of the dependent variable and that we would thereby denigrate major work that engages in this practice. We consider these arguments in turn.

Science as a Collective Enterprise

Rogowski argues that we would reject several classic case studies in comparative politics. We think he misunderstands these studies and misses our distinction between a "single case" and a collection of observations. Consider two works that he mentions, *The Politics of Accommodation*, by Arend Lijphart (1968), and *The Nazi Seizure of Power*, by William Sheridan Allen (1965). Good research designs are rarely executed by individual scholars isolated from prior researchers. As we say in our book, "A single observation can be useful for evaluating causal explanations if it is part of a research program. If there are other observations, perhaps gathered by other researchers, against which it can be compared, it is no longer a single observation" (p. 211; see also secs. 1.2.1, 4.4.4, the latter devoted entirely to this point). Rogowski may have overlooked these passages. If we did not emphasize the point sufficiently, we are grateful for the opportunity to stress it here.

Lijphart: The Case Study that Broke the Pluralist Camel's Back

What was once called *pluralist theory* by David Truman and others holds that divisions along religious and class lines make polities less able to resolve political arguments via peaceful means through democratic institutions. The specific causal hypothesis is that the existence of many cross-cutting cleavages increases the level of social peace and, thus of stable, legitimate democratic government.

In *The Politics of Accommodation*, Arend Lijphart (1968) sought to estimate this causal effect.[3] In addition to prior literature, he had evidence from only one case, the Netherlands. He first found numerous observable implications of his descriptive hypothesis that the Netherlands had deep class and religious cleavages, relatively few of which were cross-cutting. Then—surprisingly from the perspective of pluralist theory—he found considerable evidence from many levels of analysis that the Netherlands was an especially stable and peaceful democratic nation. These descriptive inferences were valuable contributions to social science and important in and of themselves, but Lijphart also wished to study the broader causal question.

In isolation, a single study of the Netherlands, conducted only at the level of the nation at one point in time, cannot produce a valid estimate of the causal effect of cross-cutting cleavages on the degree of social peace in a nation. But Lijphart was *not* working in isolation. As part of a community of scholars, he had the benefit of Truman and others having collected many prior observations. By using this prior work, Lijphart could and did make a valid inference. Prior researchers had either focused only on countries with the same value of the explanatory variable (many cross-cutting cleavages) or on the basis of values of the dependent variable (high social conflict). Previous researchers therefore made invalid inferences. Lijphart measured social peace for the other value of the explanatory variable (few cross-cutting cleavages) and, by using his data in combination with that which came before, made a valid inference.

Lijphart's classic study is consistent with our model of good research design. As he stressed repeatedly in his book, Lijphart was contributing to a large scholarly literature. As such, he was not trying to estimate a causal effect from a single observation; nor was he selecting on his dependent variable. Harvesting relevant information from others' data, although often overlooked, may often be the best way to obtain relevant information.

By ignoring the place of Lijphart's book in the literature to which it was contributing, Rogowski was unable to recognize the nature of its contribution. Rogowski's alternative explanation for the importance of this book and the others he mentions—that "(1) all of them tested, relied on, or proposed, clear and precise *theories*; and (2) all focused on *anomalies*" (p. 469)—suggests one of many possible strategies for choosing topics to research; but it is of almost no help with practical issues of research design or ascertaining whether a theory is right or wrong. Indeed, the only way to determine whether something is an anomaly in the first place is to follow a clear logic of scientific inference and theory evaluation, such as that provided in *Designing Social Inquiry*.

Allen: Distinguishing History From Social Science

The Nazi Seizure of Power is an account of life in an ordinary German community during the Nazi seizure of power. Allen is not a social scientist: In his book, he proposes no generalization, evaluates no theory, and does not refer to the scholarly literatures on Nazi Germany; rather, he zeroes in on the story of what happened in one small place at a crucial moment in history, and he does so brilliantly. In our terms, he is describing historical detail and occasionally also conducting very limited descriptive inference. We emphasize the importance of such work: "Particular events such as the French Revolution or the Democratic Senate primary in Texas may be of intrinsic interest: they pique our curiosity, and if they were preconditions for subsequent events (such as the Napoleonic Wars or Johnson's presidency) we may need to know about them to understand those later events" (p. 36).

In our view, social science must go further than Allen. The social scientist must make descriptive or causal inferences, thus seeking explanation and generalization. Indeed, we think even Rogowski would not accept Allen's classic work of history as a dissertation in political science. Allen's work is, however, not irrelevant to the task of explanation and generalization that is of interest to us. In the hands of a good social scientist, who could place Allen's work within an intellectual tradition, it becomes a single case study in the framework of many others. This, of course, suggests one traditional and important way in which social scientists can increase the amount of information they can bring to bear on a problem: read the descriptive case study literature.

The Perils of Avoiding Selection Bias

We agree with David Collier's observation that, if our arguments concerning selection bias are sustained, then "a small improvement in methodological self-awareness can yield a large improvement in scholarship" (p. 461). Indeed, because qualitative researchers generally have more control over the selection of their observations than over most other features of their research designs, selection is an especially important concern (a topic to which we devote most of our chap. 4).[4]

Rogowski believes that we would criticize Peter Katzenstein's (1985) *Small States in World Markets* or Robert Bates's (1981) *Markets and States in Tropical Africa* as inadmissibly selecting on the dependent variable. We address each book in turn.

Katzenstein: Distinguishing Descriptive Inference from Causal Inference

Peter Katzenstein's (1985) *Small States in World Markets* makes some important descriptive inferences. For example, Katzenstein shows that small European states responded flexibly and effectively to the economic challenges that they faced during the 40 years after World War II; and he distinguishes between what he calls "liberal and social corporatism" as two patterns of response. But many of Katzenstein's arguments also imply causal claims—that in Western Europe "small size has facilitated economic openness and democratic corporatism" (p. 80), and that in the small European states, weak landed aristocracies, relatively strong urban sectors, and strong links between country and city led to cross-class compromise in the 1930s, creating the basis for postwar corporatism (chap. 4).

Katzenstein seeks to test the first of these causal claims by comparing economic openness in small and large states (1985, table 1, p. 86). To evaluate the second hypothesis, he compares cross-class compromise in six small European states characterized by weak landed aristocracies and strong urban sectors, with the relative absence of such compromise in five large industrialized countries and Austria, which had different values on these explanatory variables. Much of his analysis follows the rules of scientific inference we discuss—selecting cases to vary the value of the explanatory variables, specifying the observable implications of theories, seeking to determine whether the facts meet theoretical expectations.

But Katzenstein fudges the issue of causal inference by disavowing claims to causal validity: "Analyses like this one cannot meet the exacting standards of a social science test that asks for a distinction between necessary and sufficient conditions, a weighting of the relative importance of variables, and, if possible, a proof of causality" (p. 138). However, estimating causal inferences does not require a "distinction between necessary and sufficient conditions, a weighting of the relative importance of variables," or an absolute "proof" of anything. Katzenstein thus unnecessarily avoids causal language and explicit attention to the logic of inference which results. As we explain in our book, "Avoiding causal language when causality is the real subject of investigation either renders the research irrelevant or permits it to remain undisciplined by the rules of scientific inference" (p. 76).

Remaining inexplicit about causal inference makes some of Katzenstein's claims ambiguous or unsupported. For example, his conclusion seems to argue that small states' corporatist strategies are responsible for their postwar economic success. But because of the selection bias induced by his decision to study only successful cases, Katzenstein cannot rule out an important alternative causal hypothesis—that any of a variety of other factors accounts for this uniform pattern. For instance, the postwar international political economy may have been benign for small, developed countries in Europe. If so, corporatist strategies may have been unrelated to the degree of success experienced by small European states.

In the absence of variation in the strategies of his states, valid causal inferences about their effects remain elusive. Had Katzenstein been more attentive to the problems of causal inference that we discuss, he would have been able to claim causal validity in some limited instances, such as when he had variation in his explanatory and dependent variables (as in the 1930s analysis). More importantly, he would also have been able to improve his research design so that valid causal inferences were also possible in many other areas.

Rogowski is not correct in inferring that we would dismiss the significance of *Small States in World Markets*. Its descriptions are rich and fascinating, it elaborates insightful concepts such as liberal and social corporatism, and it provides some evidence for a few causal inferences. It is a fine book, but we believe that more explicit attention to the logic of inference could have made it even better.

Bates: How to Identify a Dependent Variable

Rogowski claims that Robert Bates's purpose in *Markets and States* was to explain economic failure in tropical African states and that by choosing only states with failed economies and low agricultural production, Bates biased his inferences. If agricultural production were Bates's dependent variable, Rogowski would be correct, since (as we describe in *Designing Social Inquiry* and as elaborated by Collier), using—but not correcting for—this type of case selection does bias inferences. However, low agricultural production was, in fact, not Bates's dependent variable.

Bates's book makes plain his two dependent variables: (1) the variations in *public policies* promulgated by African states and (2) differences in the *group relations* between the farmer and the state in each country. Both variables vary considerably across his cases. Bates also proposed several explanatory variables, which he derived from his preliminary descriptive inferences. These include (1) whether state marketing boards were founded by the producers or by alliances between government and trading interests, (2) whether urban or rural interests dominated the first postcolonial government, (3) the degree of governmental committment to spending programs, (4) the availability of nonagricultural sources for governmental funds, and (5) whether the crops produced were for food or export. These explanatory variables do vary, and they helped account for the variations in public policy and state-farmer relations that Bates observed.

As such, Bates did not select his observations so they had a constant value for his dependent variable. Moreover, he did not stop at the national level of analysis, for which he had a small number of cases and relatively little information. Instead, he offered numerous observable implications of the effects of these explanatory variables at other levels of analyses within each country. As with many qualitative studies, Bates had a small number of cases but an immense amount of information. We believe one of the reasons Bates's study is—and should be—so highly regarded is that it is an excellent example of a qualitative study that conforms to the rules of scientific inference. In sum, Rogowski says that Bates had an excellent book that we would reject. If the book were as Rogowski describes it, we very well might reject it. Since it is not—and indeed is a good example of our logic of research design—we join Rogowski in applauding it.[5]

TRIANGULAR CONCLUSIONS

We conclude by emphasizing a point that is emphasized both in *Designing Social Inquiry* and in the reviews. We often suggest procedures that qualitative researchers can use to increase the amount of information they bring to bear on evaluating a theory. This is sometimes referred to as "increasing the number of observations." As all our reviewers recognize, we do not expect researchers to increase the number of full-blown case studies to conduct a large-*n* statistical analysis: our point is not to make quantitative researchers out of qualitative researchers. In fact, most qualitative studies already contain a vast amount of information. Our point is that appropriately marshaling all the thick description and rich contextualization in a typical qualitative study to evaluate a specific theory or hypothesis can produce a very powerful research design. Our book demonstrates how to design research in order to collect the most useful qualitative data and how to restructure it even after data collection is finished, to turn qualitative information into ways of evaluating a specific theory. We explain how researchers can do this by collecting more observations on their dependent variable, by observing the same variable in another context, or by observing another dependent variable that is an implication of the same theory. We also show how one can design theories to produce more observable implications that then put the theory at risk of being wrong more often and easily.

This brings us to Sidney Tarrow's suggestions for using the comparative advantages of both qualitative and quantitative researchers. Tarrow is interested specifically in how unsystematic and systematic variables and patterns interact, and seems to think that principles could be derived to determine what unsystematic events to examine. We think that this is an interesting question for any historically-sensitive work. Many unsystematic, nonrepeated events occur, a few of which may alter the path of history in significant ways; and it would be useful to have criteria to determine how these events interact with systematic patterns. We expect that our discussions of scientific inference could help in identifying which apparently random, but critical, events to study in specific instances, and we are confident that our logic of inference will help determine whether these inferences are correct; but Tarrow or others may be able to use the insights from qualitative researchers to specify them more clearly. We would look forward to a book or article that presented such criteria.

Another major point made by Tarrow is that all appropriate methods to study a question should be employed. We agree: a major theme of our book is that there is a single unified logic of inference. Hence it is possible effectively to combine different methods. However, the issue of triangulation that Tarrow so effectively raises is not the use of different logics or methods, as he argues, but the triangulation of diverse *data sources* trained on the same problem. Triangulation involves data collected at different places, sources, times, levels of analysis, or perspectives, data that might be quantitative, or might involve intensive interviews or thick historical description. The best method should be chosen for each data source. But more data are better. Triangulation, then, is another word for refering to the practice of increasing the amount of information to bear on a theory or hypothesis, and that is what our book is about.

Notes

The table of contents, preface, and chapter 1 of *Designing Social Inquiry* are available via Gopher from hdc-gopher.harvard.edu.

1. To clarify further, we note that the definition of an "experiment" is investigator control over the assignment of values of explanatory variables to subjects. Caporaso emphasized also the value of random assignment, which is desirable in some situations (but not in others, see pp. 124–8) and sometimes achievable in experiments. (Random selection and a large number of units are also desirable and also necessary for relatively automatic unbiased inferences, but experimenters are rarely able to accomplish either.) A "quasi-experiment" is an observational study with an exogenous explanatory variable that the investigator does not control. Thus, it is not an experiment. Campbell's choice of the word "quasi-experiment" reflected his insight that observational studies follow the same logic of inference as experiments. Thus, we obviously agree with Campbell's and Caporaso's emphases and ideas and only pointed out that the word "quasi-experiment" adds another word to our lexicon with no *additional* content. Its a fine idea, much of which we have adopted; but it is an unnecessary category.

2. Telling researchers to "choose better theories" is not much different than telling them to choose the right answer: it is correct but not helpful. Many believe that deriving rules for theory creation is impossible (e.g., Popper, Feynman), but we see no compelling justification for this absolutist claim. As David Laitin correctly emphasizes, "the development of formal criteria for such an endeavor is consistent with the authors' goals."

3. Lijphart also went to great lengths to clarify the precise theory he was investigating, because it was widely recognized that the concept of pluralism was often used in conflicting ways, none clear or concrete enough to be called a theory. Ronald Rogowski's description of pluralism as a "powerful, deductive, internally consistent theory" (p. 10) is surely the first time it has received such accolades.

4. Selection problems are easily misunderstood. For example, Caporaso claims that "if selection biases operate independently of one's hypothesized causal variable, it is a threat to internal validity; if these same selection factors interact with the causal variable, it is a threat to external validity" (p. 9). To see that this claim is false, note, as Collier reemphasizes, that Caporaso's "selection factors" can also be seen as an omitted variable. But omitted variables cannot cause bias if they are independent of your key causal variable. Thus, although the distinction between internal and external validity is often useful, it is not relevant to selection bias in the way Caporaso describes.

5. Subsequently, Bates pursued the same research program. For example, in *Essays on the Political Economy of Rural Africa* he evaluated his thesis for two additional areas—colonial Ghana and Kenya (1983, chap. 3). So Bates does exactly what we recommend: having developed his theory in one domain, he extracts its observable implications and moves to other domains to see whether he observes what the theory would lead him to expect.

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POLITICAL THEORY

The War Lover: A Study of Plato's *Republic*. By Leon Harold Craig. Toronto: University of Toronto Press, 1994. 439p. $65.00.

This book offers a novel and provocative interpretation of Socrates' defense of the philosophic life in the *Republic*. While the book contains a wide-ranging discussion of the dialogue as a whole, Craig's central thesis is that the *Republic* "amounts to a defense of the philosopher against the all-too-common conception of him as a soft, indolent, pacifistic, irreverent, politically useless, effeminizing corrupter of young men, by example if not by active intent" (p. 13). To support this thesis, Craig focuses on "the curious identification of philosophy and warfare" that runs throughout the dialogue (p. 21). On the basis of a detailed analysis of Socrates' psychology of the spirited *war lover* and the passionate *wisdom lover*, Craig concludes that according to the *Republic*, those who are best suited for the pursuit of wisdom are to be found among the ranks of those well suited for war; that the philosopher requires many of the virtues and passions of the warrior; and hence that while most of those who are attracted by a life of thought "tend to be by nature timorous, pacifistic, tender-minded, and excessively rationalistic," as well as "utopian" and "sentimental," the true philosopher "is distinguished first of all by a great, strong, and bold spirit," and the truly philosophic life requires "mental strength" and "what is even more basic, exceptional *spiritual* strength" to persevere and to overcome one's fear of "the possibly tragic character of the truth itself" (pp. 228–30, 271–72; emphasis original).

Craig's apparent intention in this book is to argue against the popular and scholarly conception of the philosophic life as "the *passive* life of contemplation" and as the "impartial, disinterested, cosmopolitan search for eternal truth" (p. 19, emphasis original) and in favor of the conception of "the pursuit of wisdom as the greatest of contests, and the self-sufficiency of philosophy as the most powerful, masterful, free, and beautiful of lives" (pp. 288–89). One might suspect that Craig is also attempting to defend the Socratic way of life against the Nietzschean charge that it is a soft, decadent, and ignoble way of life. Yet it is not at all clear whether Craig means to defend Socratic rationalism against Nietzsche's critique or whether he means to suggest that there is a fundamental kinship between these two seemingly opposed thinkers, for, especially in his notes, Craig seems to suggest that Socrates and Nietzsche may not ultimately differ in their understanding of the relationship between philosophy and war (see esp. pp. 350–53, 426).

One of the virtues of this book is that it not only provokes many objections but also anticipates and addresses a number of them. While Craig contends that of the three parts of the soul identified by Socrates in the dialogue (i.e., reason, spirit or spiritedness, and desire), the true philosopher must possess both great reason and great *spiritedness*, he readily acknowledges that the spirited love of war can come into conflict with the love of wisdom and that it can even lead to a blind fanaticism and a deadly hostility to reason. Craig argues, however, that Socrates makes a vital distinction between the spirited love of honor (which is lowly and stubborn and in which is seated the love of one's own, resentment, and spite) and the spirited love of victory (which is noble and educable, which takes pleasure in self-mastery, and which loves beauty and excellence). Craig does admit that even the lovers of victory are "naturally predisposed to treat dialogical arguments as contests with opponents, battles to be won," rather than as occasions to learn the truth from possibly wiser interlocutors (p. 245). Nevertheless, Craig insists that the noble love of victory is an essential part of the philosopher's soul and that the love of war can be "sublimated" into a love of wisdom (p. 271).

Even though Craig's argument is extraordinarily rich and thought-provoking, it is not altogether persuasive. He does not clearly explain how the psychic transformation from war lover to philosopher occurs. He does not consider Socrates' incisive critique of the superlatively spirited Achilles in book III. Nor does he discuss the seemingly crucial fact that Socrates himself appears to be remarkably unspirited. Craig's book leaves one wondering not only whether he does not overestimate the importance of spiritedness in the philosophic soul but also whether he does not underestimate the importance of eros in such a soul. For, in books V and VI of the dialogue, Socrates explicitly describes the philosopher as an erotic, not as a spirited, human being. Craig argues persuasively in this book that the philosopher is a profoundly passionate human being and hence is not merely passive or disinterested. But one wonders whether the sovereign passion of the Socratic philosopher is not more akin to the needy longing of a lover than to the proud spirit of a warrior.

The greatest contribution of this book lies in its vivid and engaging account of Plato's political psychology. Craig carefully traces the parallel between the just city and the soul of the just human being. He also shows, often quite brilliantly, how the dialogue's thematic account of human psychology and its dramatic presentation of Socrates' various interlocutors shed light on one another. In this way, Craig's interpretation does considerable justice to both the arguments and the drama of the *Republic*.

Craig is less impressive in his account of Socrates' response to Glaucon and Adeimantus' powerful objections to the just life. He tends to assume too quickly what, for Socrates, occupies an entire discussion in demonstration, namely, the superiority of the just life to the unjust life. Nevertheless, this spirited and stimulating book successfully convinces the reader of the richness of Plato's political psychology and of the importance of reexamining the meaning of Socratic political philosophy.

Davidson College PETER J. AHRENSDORF

Catholicism and Liberalism: Contributions to American Public Philosophy. Edited by R. Bruce Douglas and David Hollenbach: Cambridge University Press, 1994. 352p. $59.95.

Based on a Georgetown University conference, this excellent anthology deals with the historical and philosophical relationship between Catholicism and liberalism. All of the articles are sound, some profound, and collectively they complement and enrich the theme. They are the products of the most prominent academic notables working this vineyard—Mary Segers, Paul Sigmund, David Tracy, and Peter Steinfels, to name a few.

The volume's central theme seems to be the admonition that there is neither a Catholic nor a liberal monolith—that both manifest themselves in a variety of versions. Second, despite the often tragic relationship, both could benefit from dialogue and cross-fertilization. Catholicism offers liberalism a legacy of addressing the ultimate questions, an emphasis on the common good, an organic orientation, a deference to family, respect for mediative institutions, and experience with subsidiarity. Liberalism offers Catholicism its dedication to the rule of law, its emphasis on procedural concerns and human rights, and its flexibility in addressing contemporary concerns.

Catholicism and liberalism seem to differ substantially in approaching change. Catholicism is a popular, international religion that embraces close to a billion followers. An Incarnational/sacramental religion that perceives nature and grace as intertwined, it unapologetically penetrates the political arena. It has inspired a vast network of political parties, interest groups, charitable institutions, and self-help organizations. Catholicism, like most religions, is conservative in ethos. It traces its linage through two thousand years and consistently focuses on past events for purposes of legitimacy, inspiration, and mobilization. It interprets the present in terms of an organic tradition and defers to a cyclical ceremony. In the Catholic lexicon, terms such as authority, dogma, and ritual are not pejorative. Consequently, Catholicism fosters change that is organic, incremental, and consistent with precedent. It often stems from the systemic attempt to reconcile the priestly and prophetic roles, as well as the balancing act among pope, bishops, theologians, and laity. These tensions, linked with differences among the geographic national churches, contribute to Catholic change.

Catholicism is a highly syncretic religion in its absorption of culture. It perceives New Testament Christianity as the seed, rather than the model. It expects and welcomes a kind of Hegelian "unfolding of the absolute" as it encounters, and attempts to incorporate, new cultures. Historically, Christianity moved out of its Jewish environment, utilized Greek philosophy to define its Christological/Trinitarian dogmas, coopted Roman law and bureaucracy to construct its canon law and church structure, and adapted Stoic philosophy to its own egalitarian stance. To update the process, the Vatican II Council (1962–65) could be interpreted as an attempt to absorb features of the Protestant Reformation and secular liberalism.

Liberalism also enshrines change, but in a more mechanical fashion. Nineteenth-century liberalism, with its individualism, hostility to government, and laissez-faire economics, differs substantially from twentieth-century social welfare liberalism. Current American liberalism, with its upper-middle-class agenda, differs on concern, priority, and focus from the liberalism of the old Democratic ascendancy. Liberalism seems capable of reinventing itself for each new generation, preserving only the concern for individual autonomy.

It is far from certain that an encounter with liberalism need be beneficial to Catholicism or, indeed, to any traditional religion. Church history is suggestive on the question. The Catholicism that rejected the existent liberalism with the Councils of Trent and Vatican I enjoyed subsequent periods of significant dynamism. Impressive revitalization followed each conservative council with new religious orders, vigorous missionary expansion, and cultural creativity. Although the results are not yet in, it is far from certain that the more progressive Vatican II Council will prove propitious.

The American Protestant case is also of relevance. Early in this century, American Protestantism split into liberal and conservative components. The mainstream denominations embraced the higher biblical criticism and the social gospel, while deemphasizing Christianity's mystical/spiritual nature. The conservative dissenters rejected the novel and reaffirmed orthodoxy. Most Protestant institutions sided with the liberals, while the conservatives (evangelicals and fundamentalists) were marginalized where they concentrated on regrouping and building community. Today, the mainstream liberal denominations continue their 30-year decline, while the conservative denominations continue to grow. Liberalism did not prove a successful formula for American (or European) Protestant revitalization.

While questioning the benefits of liberalism for Catholicism, one might also question the benefits of Catholicism for liberalism. The Catholic concerns cited so often by the authors as possible supplements to liberal emphasis might be equally secured from a judicious reading of Aristotle and Burke. It is doubtful that liberalism will humanize Catholicism or that Catholicism will infuse liberalism with a soul. Perhaps the best that partisans of reconciliation can expect is something akin to the Democratic ascendancy period, when the liberals demonstrated some sensitivity toward traditional religion and the Catholic voters (consistent with their social and economic interests) rewarded them with political support. This pragmatic alliance is not without interest to the politically astute, since Catholics, combined with evangelical Protestants, form over half the American electorate. A cursory examination of denominational publications suggests that a Catholic-evangelical alliance is far more likely than a Catholic-liberal one.

University of Arizona DANIEL J. O'NEIL

Intimacy and Spectacle: Liberal Theory as Political Education. By Stephen L. Esquith. Ithaca: Cornell University, 1994. 285p. $39.95.

Ideally, what dispositions ought to characterize ideal liberal democratic citizens? What should we learn to be good citizens? For their part, liberals promote an education in toleration and self-regulation; communitarians seek to teach us about our shared past, and democrats emphasize a training in discussion and participation. In *Intimacy and Spectacle,* Stephen L. Esquith comes down clearly on the democratic side of the debate with his own arresting and important contribution which emphasizes an education which speaks to the nature of power and the relationship of citizenship to power.

The author argues that a liberal education leads to consumerism and clientism, something he finds in Hobbes, Kant, J. S. Mill and Rawls. Esquith hardly clams these four liberals self-consciously seek to promote such dispositions but that these are the logical consequences of an education which, he believes, promotes stability, defers to experts, and fears the capacity of ordinary citizens to formulate a view of citizenship. For his part, Esquith initially reaches to Thucydides and Aristotle to provide the argument that civic education should prepare "citizens to perceive power relations and respond to them justly together" (pp. 91–92) and then later turns to an extended argument about an Emersonian education.

When Esquith looks at Hobbes, he finds a radical separation between the public and private and silence about the "the judgment to balance private loves and affections against public duties" (p. 113). For his part, Kant is seen encouraging a "political deafness among independent citizens" who are concerned about their property claims which, in turn, leads to "administrative and professional mediation that . . . may alienate citizens from democratic politics" (p. 133).

Esquith finds Millian education relying on a Socratic foundation emphasizing dialogue and a Platonic grounding stressing "expertise in matters of political economy and the need for heroic, inspiration grandeur" (p. 143). Here, Esquith's reading becomes highly contestable. Strong participatory democrats such as Pateman and Barber would find the argument that Mill paves the way to clientism to be a curious reading. And what are we to make of the charge Mill is a forerunner of consumerism when he complains bitterly and constantly about the "money getting and money having" of his generation? Part of the answer, according to the Esquith, is found in Mill's reliance on experts, something the author finds in many Millian texts, particularly in those about the family. Contrary to earlier conventional readings of the Millian family, Esquith does not see it as a training ground for participation and wide-ranging discussion but as a place where "All the subtle tones, the nuanced and hushed voices, and the sighs and exasperations that bind families together have been scrubbed away" (p. 170).

Esquith's Rawls "orients citizens toward existing structures of expert authority in the spirit of moderate reform and accommodation" (p. 174). Some communitarian critics of Rawls have taken his theory to privilege proceduralism over such goods as community or civic virtue; for Esquith, the casualty is democracy and he wants to offer a democratic education where "neither substantive justice nor procedural fairness gains a permanent advantage over the other, and to do this in a sometimes violent, often unpredictable atmosphere" (p. 205).

To counter the consumerism and clientism Esquith detects in liberalism, we are offered Emerson as the exemplary democratic educator, even though he is highly suspicious of democratic politics as practiced and insensitive to economic failure. What qualifies Emerson is not the individualism Kateb recently celebrated in *The Inner Ocean* (1993). Esquith wants "Emerson's public intellectual" to disturb silences, challenge agreements, question power in translations that "people can hear and respond to so that they are not awestruck" (p. 253). The public intellectual sees action as bipolar and dualistic, defies fixity or stasis, and uses metaphors that are fluid and energetic to describe the ideal society.

But like his liberals, Esquith's Emerson has his own silences. Sometimes they are the same voids Esquith criticizes in liberals (as regarding families, women and the poor) and sometimes they are Esquith's omissions. Why, one wonders, is it appropriate to castigate J. S. Mill who challenged the conventional norms regarding the family within the context of nineteenth century liberalism but then silently pass over Emerson's silence about the same subject? Why are the partial silences of Mill more disturbing than Emerson's deafness? The reader is also curious about what substantively follows Emerson's (and Esquith's) critique of power.

Curiosity is raised because Esquith's interests change. The consumerism and clientism that preoccupied him earlier in the book fade as the issue of race and the national security state loom large in the closing pages. But more is involved here. Emerson had a very uneasy relationship with wealth; for all of his criticisms of consumerism, he believed wealth was necessary and was silent about limiting its acquisition or distribution. Silence about institutional arrangements and distributional issues are embedded in both Emerson's and Esquith's projects (although important to Mill and central to Rawls).

This can be seen in the case the author offers of the black welfare client finding her own voice. Esquith offers a moving narrative of someone who is able to assert herself against a well-meaning clientism and command her own identity. While the articulation of her own position is the essential beginning of an understanding and critique of power, one wonders what happens when she leaves the benefits office and returns to her public housing apartment. Rawls may not have the last word to say on this point but he offers several important ones in his second principle of justice which at least challenges the inequalities resident in contemporary society. Rawls is unsatisfied just to hear her Emersonian voice and then wave goodby as she boards the bus back home to fend for herself.

The flaws in the readings of Mill and Rawls do not detract from Esquith's larger and important argument that civic education is about how we understand, locate, and respond to power, how liberalism often promote silences about power, and how individualist solutions played out in consumerism deplete politics of much substance. Esquith relies on an Emersonian admonition not only to be skeptical and questioning about power and authority but also to shake our own silence and to speak out against their abuses.

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RONALD J. TERCHEK

Citizens Without Sovereignty: Equality and Sociability in French Thought, 1670–1789. By Daniel Gordon. Princeton: Princeton University Press, 1994. 270p. $39.50 cloth.

Can diverse individuals, relying neither on the identities given them by their rank or estate, nor on the ontological foundations typifying traditional forms of interaction, meet under equal conditions to exchange views while maintaining polite and moral rules? The question is topical and particularly vital in our own time as modern forms of personal achievement and political organization continue to unravel and fragment and new proposals for human intercourse are heatedly debated. A historical precedent for this question can be found in the writings of early Enlightenment thinkers and it is the virtue of this superb book that it draws our attention to this possibility.

The thesis of the book is that the idea of sociability (the social, society) arose in the seventeenth century as an alternative to the choice between an absolutist idea of sovereignty and a populist democratic or republican idea of freedom. The interpretation of ourselves as sociable beings is, Gordon argues, a historical construction. It was a response to a political sphere monopolized by the monarch and it attempted to secure a place for voluntary, meaningful, dignified activity, where civility and politeness among equals permitted discussions concerning order and good policy. The author traces the period from Louis XIV to the French Revolution for the emergence of the sociable, finding in the chief intellectuals of the time less evidence of proto-revolutionary zeal, than a desire to find a sphere which could co-exist with a corporate regime of privileges. Gordon's interest in the emergence of the social follows the well-trodden path of thinkers like Hannah Arendt, Jacques Donzelot, Richard Sennett, and Jürgen Habermas (albeit with far greater historical precision and with a plethora of rich, suggestive footnotes), though his assessment of the significance of the "social" is far more optimistic than that of the first two thinkers and far less optimistic than that of the latter two.

Gordon's narrative opens with Louis XIV's commissary of the Paris police, Nicolas Delamare, being sent to restore peace to Troyes, a city in revolt. Delamare subdued the dissidents by ordering changes in the trade and distribution of grain, buying their allegiance by assessing and responding to their essential needs. Delamare's actions were governed by ideas of a "well-policed" state under administrative regulations, which he outlined in his *Traité de la police* (1729), and represent the beginning of a progressive governmental enterprise to manage public welfare. Gordon sees the term "police," with its connotations of refined and cultivated order, as a significant antecedent to civilisation, and a key to the emergence of the social sphere.

Gordon's investigations are thorough and comprehensive. He examines the growing perception among Enlightenment thinkers that sociability was not merely latent, needing coercive sovereign power to be elicited (as natural-law thinkers had proposed), but as a constitutive feature of social life and thus as the basis of social (though not political) equality. He tracks the rise of "social virtue," as a middle ground between self-interest and benevolence, and thus between the traditional estates and a democratic polity. He outlines the way in which the idea of "society" was used to correct the excesses of the new commercial regimes by promoting self-vigilance and thus rendering the need for coercive sovereign power superfluous. He looks at the reassessments of religion in light of its social utility, as for example in the transposition of holiness into social gentleness. He offers a creative use of Erving Goffman's idea of "transformation rules" to analyse how the performance of sociable conversations permitted a suspension of ordinary political hierarchies, as if its participants were in fact equals. He proposes that the social was not so much a "public sphere in apolitical form" as if, with Habermas, it was an ersatz for political action, but was a deliberate invention to achieve the high ideals that the political would not allow to be realized. He makes a sensible case that the social afforded an ethical, and not merely aesthetic, dimension. And finally, he depicts how the age of sociability came to an end, caught between the clash of its form and substance, the urbane exchange of public opinion transformed into the truths of revolutionary politics with society as the object of administrative reform.

In Gordon's discussion of this unhappy conclusion to an otherwise credible experiment, his analysis reveals a kinship to studies like Karl Löwith's *Meaning in History* (1949) or Hans Blumenberg's *The Legitimacy of the Modern Age* (1983), which similarly find modernity's highest aspirations dashed when philosophies of universal history were adopted. In Gordon's view, sociability was falsely annexed to a false transposition of religious faith's transcendent power into the new ecumenic role that reason was to play in re-shaping the world. Once cosmopolitanism and polite manners were seen as the height of civilisation, and the polite speech of the salon transformed into dreams of rationalizing the public sphere, the moral promise of the Enlightenment had derailed into various utopian fantasies of "heavenly cities." Unlike Löwith, although more like Blumenberg, Gordon allows us to linger awhile in the interstice between ancient absolutism and messianic administrative "democracy" permitting his reader to enjoy the possibility of a moderate enterprise not emasculated by despair and to entertain the importance of a meaningful social activity not driven by presumption.

Nonetheless, perhaps there is less to enjoy about this initiative than Gordon lets on. Gordon sees "sociability" primarily in a positive light. He thereby breaks with two formidable writers, Hannah Arendt and Michel Foucault, who are highly critical of the social. Gordon focuses exclusively on the voluntary, mannerly, civil character of the social, its potential to cultivate philosophical, aesthetic, and moral concerns, and its ability to avert the idea and processes whereby individuals and society are taken as phenomena needing to be disciplined through administrative norms. The other side of the story is vividly portrayed in Hannah Arendt's *The Human Condition* (1958). Her argument is that such administration is endemic to the idea of the social, not a betrayal of it. The social, she argues, arises when one general human characteristic, our sentient existence, is elevated to significance beyond the more important fact of our evident plurality. Unlike the political world that had been characterized by a genuine diversity of interests, the social realm has a statistical uniformity arising from measureable and predictable regularities in human behaviour. "Government," the most "social" form of politics, is inherently an administration of social norms because humans are known only by their sentient species-being. Moreover, Arendt argues, the social is a highly unstable form. In time when society inevitably appears as inhospitable to the human heart, individuals turn to their private lives for satisfaction, nurturing intimacy and nursing grievance. For Arendt, the situation is a highly volatile one, for the lonely heart is receptive to innumerable phantoms and is easily manipulated. And this, she argued, set the stage for mass society in the twentieth century and its vulnerability to being agitated and mobilized through propaganda to violence and destruction.

The second concern is raised in Michel Foucault's essay "On Governmentality" (1979) and by Jacques Donzelot in his book *L'Invention du Social* (1984). What are the conditions, material and epistemological, they ask, which must prevail for the "social" to exist? Gordon highlights positively the many voluntary institutions such as the academies, clubs, lodges, and libraries which furthered the idea of sociability and the projects of public welfare and progress it underwrote. He refers, for example, to the salon's function of socializing people to updated norms or the Academy's role of harmonizing a monarchical institution with liberal reform. Generally, Gordon accepts the idea that the shift from royal will to collective reason overcame the domination of the past. Donzelot and Foucault see the matter differently. These institutions played a major role in regularizing a form of positivistic knowledge which was used to manage and normalize populations. Although power was no longer paternalistic, the coerciveness of "fraternalistic" relations was as great, if not greater. Gordon sees individuals capacity to police themselves as an improvement on sovereign control. Foucault would demur, pointing out that the new social forms of vigilance, surveillance, and population management supplying the data for the modern social sciences was, in many cases, a more insidious and efficient means of controlling individuals, for it preempted and subverted traditional forms of resistance under the banner of progress in scientific reason.

Finally, Gordon adopts the maddening timidity endemic to professional historians, counseling caution, if not utter inaction, when evoking past ideals for assessing present reality. He concludes on a modest, nearly pessimistic, note, abjuring the opinions of Sennett and Habermas that urbane, polite discussion is a viable paradigm for modern political resistance to contemporary bureaucracy and social rivalry. Gordon argues that it is as hard to think of a world without the legal and institutional checks of the state, as it is to believe that the taste for urbane conversation could prevail in an age given over to television and mass communication. Moreover, such efforts are fantastic, he writes, because they assume that the ideal enjoyed a consensus, which it did not. History, as Gordon points out, is "a horde," and speculating on reshaping the world by hypostatizing one ideal, is an act of "poetry," if not dangerous revolution. Against this prudent advice, one could say, however, that were it not for our capacity to act from time to time on the inspiration given us by the Muses, the symbolic forms which confer meaning on our individual and political lives would stagnate, cut off from those vital processes of renewal upon which the world relies and to which our capacity for action inclines us.

Carleton University PETER C. EMBERLEY

Hegel's Social Philosophy: The Project of Reconciliation. By Michael O. Hardimon. Cambridge: Cambridge University Press, 1994. 278p. $54.95 cloth, 17.95 paper.

In *Hegel's Social Philosophy,* Michael Hardimon tries to make sense of one distinctive feature of Hegel's social and political philosophy—the pursuit of *reconciliation,* the attempt to make alienated modern individuals at home in their social world. Hardimon rightly notes that Hegel was unique in making this "project of reconciliation" the most important goal of moral and political philosophy. Nevertheless, reconciliation has rarely been an explicit and central theme in studies of Hegel, probably because we are so much more comfortable talking about alienation and its implications. To make up for this shortcoming, Hardimon's book both reconstructs Hegel's general idea of reconciliation and explains its application in *The Philosophy of Right.*

The first part of the book (the analysis of the Hegelian idea of reconciliation) is, to my mind, the most valuable. Hardimon here patiently constructs a persuasive and precise account of Hegel's understanding and justification of the project of reconciliation. Doing so is quite an achievement and not only because of the legendary difficulties of Hegel's conceptual vocabulary; for although Hegel relies on the idea of reconciliation throughout his mature moral and political thought, he never presents a sustained or systematic account of the subject. Thus, Hardimon must range widely through the Hegelian corpus, from *The Science of Logic* to the Berlin lectures on religion and aesthetics, in order to find all of the elements of this key idea in Hegelian social philosophy. Hardimon locates and synthesizes these passages in clear, precise prose, without any of the mind-numbing jargon that accompanies many studies of Hegel. His reference point is always that of an intelligent observer, eager to learn something new and distinctive from Hegel, rather than that of the devoted disciple.

The second part of the book, the account of the application of the idea of reconciliation in *The Philosophy of Right,* is less successful. It contains a helpful account of the way in which Hegel associates individuality and social membership with each other. But the interpretations of *The Philosophy of Right,* while generally sound, are not particularly original or enlightening.

Part of the problem here lies in the restrictive focus on the idea of reconciliation. Hardimon acknowledges that for Hegel, "*freedom* is the greatest good, and that freedom and being at home in the social world are coextensive" (p. 119). But he does not follow up on the implications of this statement: that the project of reconciliation must ultimately aim at demonstrating that the seemingly constraining aspects of modern social existence are, in fact, manifestations of our freedom. *The Philosophy of Right* is an attempt to show how subjective and objective freedom have been satisfactorily realized in the main practices and institutions of modern society. The success of this demonstration determines the success of the project of reconciliation. Much of Hardimon's attempt to read *The Philosophy of Right* as an effort at reconciliation seems flat and unpersuasive, because he fails to explain why Hegel thought that such seemingly alienating institutions such as constitutional monarchy and bureaucracy are essential to the full realization of human freedom in the world.

Another limitation of the book is the relative lack of attention to the kind of alienation that Hegel is seeking to overcome. Here, a little more attention to Hegel's own development would have been helpful. Hardimon is so anxious—rightly—to dissociate Hegel's idea of reconciliation from the idea of resignation that he seems unwilling to explore what one must give up to accept it. Hardimon notes that for Hegel, the Greeks had a different way of being at home in the world, one that Hegel deemed inferior because of its lack of subjective freedom. What he does not note is that for the young Hegel and many of the people whose alienation Hegel is addressing, this Greek way of being has many satisfactions that a modern way of life simply cannot match. Part of Hegel's efforts at reconciliation consist in trying to show that while subjective freedom can make a home for itself in the objective world, it cannot be combined with the kind of home for which he and so many others envied the ancient Greeks. Hegelian reconciliation may not be a form of resignation, but it certainly has its regrets.

However, readers looking for a relatively painless and jargon-free introduction to the key arguments of *The Philosophy of Right* will find Hardimon's book quite useful. The writing throughout is always clear and precise, even when dealing with some of Hegel's most complex and ambitious ideas. That clarity and the careful reconstruction of Hegel's general idea of social reconciliation are the book's chief virtues.

University of Wisconsin, Madison BERNARD YACK

The Mind of John Locke: A Study of Political Theory in its Intellectual Setting. By Ian Harris. Cambridge: Cambridge University Press, 1994. 429p. $64.00.

The purpose of this book is to exhibit and explain the unity of Locke's thought with special reference to his idea of government, which is taken to be central to his intellectual development. The author contends that all of Locke's basic doctrines in all domains of thought were logically worked out, step by step, from his early reflections on religious toleration. His idea that religious choice had to be a matter between the individual and God, and his secular conception of the state are supposed to have committed Locke to a kind of rational individualism that he constantly attempted to deepen and expand. *The Two Treatises* may thus be understood as a nodal point of this exploration, which Locke was then led to follow up with a rationalist theory of education and a rationalist interpretation of Christianity.

This account of Locke's development as a piecemeal process of discovery may not be strictly accurate. It is much more likely that Locke was generally committed, at a rather early stage of his career, to a number of broadly liberal positions that he subsequently refined and elaborated. But this need not be a serious objection to the present enterprise, since the author's way of approaching Locke helps to highlight the inner connections of his various works.

A larger problem with this book is the limitation of its scholarship. It is not simply a brief interpretive essay of a sort that gives latitude to personal tastes and reflections but an ambitious attempt to demonstrate and explain the linkages of Locke's positions by reconstruction of their contexts. This requires a burden of scholarship that the author hardly begins to assume. There is, to begin with, virtually no reference to existing scholarship on any of the issues taken up, which means that we are not told what the problems of interpretation are and what, if anything, the present book seeks to add or to correct. And there is, second, only the most fleeting reference to Locke's political commitments as a means of explaining his theoretical positions, although these are sometimes important and revealing, especially on matters of detail.

But third, and most damaging of all, is the failure to consider some of the most important contexts for Locke's ideas. The book is selectively rich in citations of the English literature of the time but lamentably thin on certain issues and, above all, on international references. There is no mention—let alone summation—of the tradition of popular sovereignty and resistance theory going back to the sixteenth century. Yet this is the matrix from which Locke's own doctrine emerges and in the light of which its richness and uniqueness can alone be understood. All but completely absent, furthermore, is any reference to the great continental systems on epistemology and moral philosophy. The author seems largely uninformed on the intense debates of that period on the moral implications of nominalism and voluntarism without which Locke's concerns cannot be rightly understood. And there is not the slightest indication that the author is at all aware of the Socinian background to *The Reasonableness of Christianity*.

These limitations on the scope of scholarship are all the more unfortunate because the author does not mention—and often seems not to know—important secondary works in which Locke's connections to the intellectual currents of his time are taken up. This leads, predictably, to a number of glaring errors or omissions. Poorly informed as to the debate on voluntarism, the author is unable to find in Locke any strictly rational criterion for determining God's law. He thus misses that pivotal passage in paragraph 6 of the *Second Treatise* where Locke derives the law of nature from the status of humans as God's property. And apparently unaware of its Socinian background, he completely ignores the antitrinitarian motif of *The Reasonableness of Christianity*.

But such egregious difficulties are exceptional. The more pervasive weakness is a certain looseness of connections and a reliance on conjecture where precision and demonstration would be possible, as well as a certain failure to appreciate seemingly small details that become revealing when placed in their larger context. Yet this is mainly to say that *The Mind of John Locke* is incomplete and unfulfilled, not that it is simply wrong or misleading. It is an earnest effort, and it generally conveys the sense of Locke's intentions.

Columbia University JULIAN H. FRANKLIN

The Idea of a Liberal Theory: A Critique and Reconstruction. By David Johnston. Princeton, NJ: Princeton University Press. 204p. $29.95.

The flood of literature sometimes derisively referred to as the Rawls/Nozick industry shows no signs of slowing. David Johnston enters the lists to champion an unabashedly cosmopolitan view—*humanist liberalism*—that focuses on promoting human agency for any and all people in any and all societies. He concedes that he has in place only a rudimentary sketch.

A political theory, Johnston suggests, supplies the grounds of social criticism, not just a design for the state. It picks out what matters; like a picture, it foregrounds some considerations and obscures others. This suggestive image in hand, Johnston embarks on a hasty tour of recent theories of liberalism—Nozick standing in for a rights-based liberalism, Raz for a perfectionist liberalism, Rawls for political liberalism. Dworkin, Sen, Walzer, and others pop up along the way for less sustained treatments. In summarizing their views and generating rapid-fire lists of objections, Johnston tries to recover what he takes to be a valuable nugget that they are all getting not quite right, namely, that societies must provide people with the means to pursue their plans and projects, means ranging from mental and physical powers through liberties and opportunities, income and wealth, and status and recognition (p. 161).

Sometimes the rhetoric of the book is distracting. It is odd, for instance, to be told that "only individuals count" (an unreconstructed liberal myself, I take this kind of methodological individualism to vacillate between a trivial or vacuous view and a misguided one) and at the same time to hear repeated appeals to the health of society, a concept at home with a strongly organic account of community (see, e.g., pp. 18, 127).

There are deeper worries about the view. Johnston insists that any political theory be informationally reasonable, that it not direct our attention to fine-grained evidence that we cannot get our hands on (pp. 28–29). The criterion plays a sometimes sharply critical role in his evaluation of others' views. Yet his own view seems open to very much the same objection. I do not know how we are to go about assessing every individual's share of mental and physical powers and the like. Johnston complains that Rawls tends to reduce his primary goods to wealth; but surely the motivation for doing so is precisely that, however caricatured an index of primary goods it is, we can at least measure it.

Johnston, again, wants a radically cosmopolitan or universalist liberalism. Local culture and history matters only in filling in the details of the theory: agency might require literacy or access to electronics in one society, in another, not. But the structure of the theory is always the same. Some will generate familiar, tired, and tiring worries about "relativism," whatever that is. That aside, I fear that Johnston is in the clutches of a resolutely antisociological picture here. Worrying that status might be a positional good yet one that he holds has to be available to all, Johnston says, "A society must be *socially pluralistic*: it must include a variety of different 'fields' in which people can try to excel and enable people to choose fields that seem to them congenial to their talents" (p. 184). But this is to presuppose not just the career open to talents, equal opportunity, and the rest but, especially, a highly differentiated society. It threatens to make nonsense or impotence of the claim that all societies ought to be liberal.

Carping objections aside, the book is almost always lucid, sometimes provocative, and occasionally just plain provoking. Avid consumers of the Rawls/Nozick industry will find it worthy and may well find it a useful teaching aid for undergraduates. Others—the kind who suspect that this industry is fast heading for the rust belt—will probably find nothing here to change their dour opinion.

University of Michigan DON HERZOG

The Humanity of Thucydides. By Clifford Orwin. Princeton: Princeton University Press, 1994. 235p. $35.00.

Clifford Orwin has given us a well-crafted, beautifully written book on the "humanity" of Thucydides. This humanity, Orwin argues, is not just a matter of temperament but stems from his understanding of the human, in particular, from his understanding of the relation between justice and compulsion (p. 11). Through his mastery of detail and his sensitive understanding of the nobility, the pathos, and even the ironies of political action, Orwin gives us an immense contribution to the scholarly literature on Thucydides and, more important, a profound reflection on what it means to be human.

Orwin argues that Thucydides explores the conditions of justice as well as the difficulties of its realization in the world. Indeed, as proclaimed by the Athenian envoys at Sparta, justice is restricted by the "compulsions" weighing on human beings, who are moved to rule others out of fear, honor, and profit (pp. 46–50). But if human action is not free, as this "Athenian thesis" implies, then human beings are exculpated of blame or responsibility. Anger at injustice as well as expectation of divine retribution are inappropriate (e.g., pp. 94–96, 156–57). Orwin's work explores the extent to which Thucydides accepts this "Athenian thesis."

Orwin at first suggests that Thucydides distances himself from the Athenian thesis because it does not leave sufficient room for freedom. He points to Thucydides' distinction between those cities coerced into fighting and those which, even when motivated by fear or profit, "were free to decide" (pp. 139–40). We are therefore surprised when Orwin later explains Thucydides' distance from the Athenian thesis because "the realm of choice or of rational action for human beings is . . . even smaller" than the Athenian thesis claims. Because of their longings and hopes, "most human beings are unfree even to acquiesce in necessity," as Orwin ironically puts it (p. 201). The "perfected version" of the Athenian thesis that Orwin attributes to Thucydides stresses not simply the human subjection to necessity but the human tendency to resist necessity, especially when hoping for justice and its efficacy in the world (pp. 142, 168). Unfortunately, "necessity determines the conduct of human beings only insofar as they recognize it as such." In such cases, pride overcomes fear as it lures human beings to "foolish risks" (p. 168; see also 156, 198). To understand all is therefore to pardon all, for transgression is only error. We arrive at "a more terrible truth than that human beings are evil; namely, that they are not" (p. 203).

Three questions emerge for me from Orwin's interpretation. First, Orwin claims that "Thucydides intended [his] work to be of use to statesmen," for clarity brings "an awareness of both the means and the bounds of political accomplishment—and is useful to statesmen on both accounts" (p. 4). But does Orwin reveal the utility of Thucydides' history on *both* accounts? Inasmuch as it dwells on the limits of political action (see pp. 4–5, n. 6), does Orwin's analysis support political activity or merely remind us of its necessary bounds?

Moreover, if "Thucydidean wisdom" is as Orwin describes it, is it proper to speak, as Orwin does, of "the humanity of Thucydides"? To Orwin, Diodotus, who in Orwin's view is close to Thucydides himself (p. 162, n. 32) appears "to gaze down on Athens from an Olympian height" (p. 162). But does the peak of Athenian rationalism that Orwin sees in Diodotus (p. 205) indicate a profound humanity or an Olympian indifference to human life?

Finally, has Orwin correctly identified Thucydidean wisdom? Is that wisdom best seen in the "terrible truth" that cities and political actions cease "to command our reverence even as [they cease] to provoke our indignation" (p. 204)? Or is it better seen in Thucydides' own activity in writing his history? At the end of his book, Orwin reminds us that when Thucydides describes the plague in Athens, he mentions that he himself suffered from it. But even while doing so, he "recorded the symptoms with an eye to benefiting future patients" (p. 205). Orwin makes an intriguing comparison between Thucydides' account of the plague and his history of the war itself: Thucydides' susceptibility to the human weakness he describes constitutes a bond of sympathy with others that is the source of his benevolence, while his wisdom gives him a degree of immunity to the disease (ibid.). Although Orwin compares Thucydides' intervention in human affairs with that of Diodotus in the punishment of Mytilene, he does not see that such interventions may be at odds with Olympian gazes. When Diodotus appealed to the rational self-interest of the Athenians, he curbed their anger against Mytilene and effected a more just political action. This "peak" of Athenian rationalism, contra Orwin, lies less in accepting the weakness of reason in the world (p. 171) than the ability of reason's influence over political life.

Moreover, if the peak of Athenian rationalism is represented by Thucydides himself, it may lie in that understanding of his own humanity, that allows him to benefit others. This capacity for self-reflection is shared not only by "a few" whom Orwin likens to Thucydides in their concern with the truth (p. 205). It is shared, by Orwin's own reckoning, with the Athenians more generally, who, in their longing for a noble politics, raise the question of the relation between necessity and choice with "noble candor" (p. 196).

Orwin has given us a subtle and complex analysis of the speeches in Thucydides' work, and the noble candor he sees in some of them suggests the possibility of a kind of freedom. He thus raises the question, if only implicitly, of the relation between the speeches in Thucydides and the issue of justice and compulsion. The human capacity for self-reflection, illustrated by the speeches in Thucydides' work and fostered by the history as a whole, both distances us from the compulsions around us and underlies human actions, including political ones. As a source of freedom, it provides a ground for both reverence and indignation.

Fordham University MARY P. NICHOLS

A Future for Socialism. By John E. Roemer. Cambridge: Harvard University Press, 1994. 178p. $29.95 cloth, $16.95 paper.

Demoralized socialists and anyone on the Left experiencing a crisis of confidence should read this book. The author, a mathematical economist and a founder of analytical Marxism, uses a variety of arguments to show that socialism is still a viable alternative in the modern world. John Roemer thinks the failure of centrally planned economies should not discredit the socialist project. Economies of the Soviet type failed because they (1) allocated goods in a way that did not compel competition between producers, (2) established direct political control over production units, and (3) had undemocratic governments. All three of these problems can be corrected within a socialist system.

Roemer offers an innovative and controversial definition of *socialism*. He defines *socialism* not as an any sort of institutional structure (e.g., public ownership of the means of production) but as a kind of egalitarianism. Any system with equality of opportunity for self-realization and welfare, for political influence, and for social status is socialist. When thinking about socialism, Roemer distinguishes between long-term and short-term proposals. Long-term proposals about how a socialist society should be organized remain rather vague but are gradually being developed by political philosophers. At least one short-term proposal is much more definite—*market socialism*. *A Future for Socialism* gives a blueprint for an efficient and reasonably egalitarian version of market socialism.

Market competition is the only known social mechanism that reliably induces both technological innovation and economic efficiency. Therefore, Roemer concludes, any serious proposal for socialism in the modern world must involve extensive use of market competition.

The idea of market socialism has an impressive intellectual history stretching back a full century. The first stage in the intellectual development of market socialism involved recognition that a socialist economy must use prices for purposes of economic calculation. During the second stage, it was realized that prices appropriate for socialism could be obtained by solving a complicated system of simultaneous equations. The pioneering work of the famous Polish economist Oskar Lange opened the third stage in the development of market socialism. Lange demonstrated that actual markets, not just the calculations of a central planning bureau, would be needed to a attain a socialist price equilibrium. He also proposed a way of combining markets with central planning.

According to Roemer, the fourth stage in the development of of market socialist ideas was associated with market reforms in communist countries—Yugoslavia after 1950, Hungary after 1968, China after 1978, the Soviet Union under Gorbachev. The critique of these reforms has led to the fifth—and present—stage of market socialist thinking, characterized by elimination of all price setting by central planners and by firms operating independently of state control. After reviewing various other fifth-stage models of market socialism, Roemer launches upon a discussion of his own blueprint.

Roemer rejects worker control of production units in favor of more traditional management because he thinks worker control would be averse to taking risks and hesitant about making major technological innovations. He allows entirely private ownership of firms up to a certain size, exceeding which they would be bought out (at a fair market price) by the public sector. Roemer's conception of market socialism has three important theoretical innovations: (1) use of a coupon stock market to distribute profits and discipline firm management, (2) use of public banks to monitor performance of production units, and (3) government control of market interest rates as a means of influencing investment. All the aforementioned economic mechanisms operate in a context of complete political democracy. Roemer investigates each of these mechanisms through an imaginative application of mathematical modeling. At least in the realm of economic theory, he concludes, these procedures are equally efficient but provide more equality than the capitalist alternatives.

This is how the coupon stock market works. On reaching adulthood, a person receives a standard allocation of coupons and then uses these coupons to purchase stock in firms. Such shares can be bought only with coupons, not with ordinary money. As under capitalism, firm profits are distributed in proportion to stock ownership. People trade stocks at coupon prices, thus establishing a coupon stock market. Although complete economic equality does not exist, a handful of wealthy people cannot, since only coupons can purchase stocks, own a majority of shares. Upon death, a person's coupon portfolio returns to the public treasury. The object of the coupon stock market is to maintain economic efficiency without creating extensive economic inequality.

Any innovative model for reconstructing society invites numerous objections. Roemer acknowledges the imperfection of his proposals and asks readers to take them with a spirit of inquiry. Adopting this spirit, three criticisms occur to me. It seems difficult if not impossible to keep coupons and money entirely separate. A black market in coupons will probably develop along with an elaborate bureaucratic apparatus designed to forestall this possibility. Second, it is unclear that public banks, operating outside a capitalist context, will effectively monitor the performance of production units. A cozy relationship between bank and firm could easily develop, creating exactly those soft budget constraints so destructive to Eastern European experiments with market socialism. Third, it is not evident, despite Roemer's mathematical models, that control over interest rates provides a sufficiently powerful lever to influence investment, especially in a context of unstable demand.

One could adumbrate other problems with Roemer's model for market socialism, but this would be pointless. The model is not offered as a flawless construction but as a means of reinvigorating a vital public debate in which one side has become lethargic for lack of intellectual self-assurance. As an intervention in the public debate about what economic system we should collectively aspire toward, *A Future for Socialism* succeeds brilliantly. A focus on the main issues, theoretical imagination, distillation of complex ideas to an accessible core, plus clarity and parsimony of expression are its key strengths. Reading this book will surely bolster the confidence of socialists and might even spur foes of socialism to seek the most humane possibilities available under the capitalist economic system.

University of Colorado TOM MAYER

Radical Theories: Paths beyond Marxism and Social Democracy. By Darrow Schecter. Manchester: Manchester University Press, 1994. 205p. $69.95 cloth, $19.95 paper.

It already seems a long time since the Western triumphalism that greeted the collapse of the totalitarian regimes of Eastern Europe. New world disorder is everywhere, and many Western governments struggle with massive unpopularity on the domestic front, too. It was predictable—and indeed predicted—that Western capitalism would come under renewed scrutiny when it lost the legitimation that accrued from being vastly preferable to the only visible alternatives. At the very least, it would be tested against its own rhetoric and ideology, and it might well be found wanting.

Schecter considers not only the failure of Western capitalism to live up to its own standards but also the failure of social democracy to do much about the huge disparities in wealth, power, and life chances that characterise it. He therefore seeks to examine alternatives both to social democratic and Keynesian attempts to deal with these problems and to the state-controlled nationalization models recently discredited. The alternatives have in common a libertarian socialism, loosely defined, and an implacable hostility to Leninist totalitarianism. Beyond that, there is plenty of intramural disagreement over such matters as how much of industrial society itself should be jettisoned, how far anything resembling a state is an inevitable feature of modern society, and how important traditional trade unions are for the future. All are united, however, in the belief that if democratic values are to be taken seriously, then they must reach into society at large and not be confined to the official political sphere.

An impressively wide selection of sources is dealt with, including syndicalism, anarchism, council communism, guild socialism, workers' self-management in the former Yugoslavia, and some strains of ecosocialism. Schecter has done a service in gathering these theories and movements together in one place and demonstrating how many streams of thought there are, both historically and currently, which are by any standards radical but are also free of any taint of slavishness toward Bolshevism.

Schecter's method is historical, rather than argumentative. He tends to present the ideas of these movements with relatively little comment, at least until his brief concluding section. The material provided is full (the section on guild socialism is especially good in that respect), and there are many references for students to pursue. But most of the evaluation is simply stated or left implicit, and Schecter rarely makes clear whether secondary sources cited in the notes reflect what he has said in the text or, if not, how the two do connect.

The specter of Marx haunts these pages; but his ideas are mentioned, rather than discussed. This is a pity, because scholarship in the years ahead may well reverse his own undeserved tarring with the Bolshevik brush. His political views are, in fact, far closer to some of these theorists (especially anarchists) than either he or they would admit. More importantly, his arguments about the intrinsic nature of market society are highly germane to many of the ideas put forward in this collection. If, as Schecter suggests, the market necessarily produces winners and losers (the former being those who obey its imperatives), this will have profound implications for some well-meaning attempts to humanize market society, including market socialism and cooperative movements based on ordinary market relations. In that case, some of the movements discussed will fare much better than others in theorizing sets of human relations based on something other than exchange of commodities.

Identity politics is seen by some of these theorists as a more adequate replacement for a one-sided and outmoded categorization of human beings as either belonging to the class of manual producers or not—with all political priority given to the former. Marx's much-misunderstood theory of class is also germane here, at least if it is construed in a more plausibly structural way, to pick out all those who, in their very different roles, contribute to all the goods and services produced in society. It then retains contemporary relevance and identifies a broader and more powerful potential movement for radical change—a thought that should be congenial to some of the strains of ecosocialism discussed here.

Three final points. First, the book has a fair sprinkling of misprints. Given the author's intentions, the most consequential must occur in the statement that his aim is "to demonstrate that there exits [sic] a neglected tradition of radical socialist theory and practice which need not be abandoned" (p. 1). Second, the index is strong on names occurring in the text but weak on topics covered, and it will not be easy for readers to relocate an issue they have read about. Third, it is regrettable that George Orwell's uniquely independent first-hand account of the Spanish Civil War should have been misclassified as a novel (p. 66, n. 31).

University of Bristol KEITH GRAHAM

The Boundaries of Citizenship: Race, Ethnicity, and Nationality in the Liberal State. By Jeff Spinner. Baltimore: Johns Hopkins University Press, 1994. 230p. $39.95 cloth.

In this timely book, Jeff Spinner examines how liberal states should accommodate four types of ethnocultural groups: racial groups, immigrant ethnic groups, voluntarily isolated groups with a "starkly different way of life," and territorially concentrated national groups. According to traditional liberal accounts, the principle of *nondiscrimination* is sufficient to accommodate these groups. The role of the state is simply to prevent discrimination on the basis of race, religion, or ethnicity. Members of cultural groups are then free to maintain their distinct cultural identities and practices in the private sphere if they so desire, but they will not be helped or hindered by the state. Spinner argues that this idea of a culturally neutral state is impossible. For example, states must adopt official languages, which disadvantages those whose mother tongue is not a public language. Similarly, public holidays (e.g., Christmas) often express and perpetuate the practices of a particular group, disadvantaging others. Moreover, Spinner argues, achieving nondiscrimination is more complicated than liberal theorists recognize. Norms of nondiscrimination apply not only to government but also to civil society—corporations, schools, stores, housing, and so on. If people are discriminated against by prejudiced shopowners or real estate agents, they will be denied equal citizenship even if the state itself does not discriminate. Nondiscrimination requires civility and tolerance on the part of individual citizens. Liberal citizens must learn to interact in everyday settings on an equal basis with people who are different from themselves. Government action may be needed to ensure recognition that there are many legitimate ways of being "American," in terms of one's religion, dress, or skin color.

Spinner argues that American society has done fairly well in ensuring equal citizenship for immigrant groups but has failed dramatically for racial groups. Hence, more active measures are needed to ensure equality for African-Americans, including increased political representation, teaching black history in schools, and government subsidies to create more black-controlled institutions in civil society.

Spinner emphasizes that while a liberal society protects cultural groups from discrimination, it also limits their ability to maintain their cultural distinctiveness. Nondiscrimination in civil society means that cultural boundaries tend to break down. Members of one ethnic group will meet and befriend members of other groups and adopt new identities and practices. Conversely, practices associated with one group will become adopted by members of the larger society. Over time, liberal citizenship results in "pluralistic integration" (p. 73). This does not involve the preservation of distinct cultures (since ethnic identities weaken and incorporate aspects of the larger culture); but neither is it assimilation (since ethnic groups change the larger society as they integrate). Spinner notes two exceptions to this pattern. The first involves groups like the Amish, which voluntarily isolate themselves from the larger society and avoid participating in politics or civil society. Spinner disapproves of such groups, since the Amish evade their responsibility to help tackle the nation's problems and are organized internally in illiberal ways. But he concludes that such "partial citizens" must be tolerated so long as they do not harm people inside or outside the group and so long as members are free to leave (p. 98). The other exception involves territorially concentrated language groups like the Québécois. Since states must adopt official languages and since languages will die out if they are not used in public life, national minorities have a claim to state recognition of their language (assuming they form a large enough group within a region to sustain all the institutions of a modern society). This ensures that the Québécois do not have to integrate into the larger anglophone society in Canada. This is only a partial exception to pluralistic integration, since Spinner insists that language-based nationalism is only acceptable if the national minority itself adopts a form of pluralistic integration, according equal citizenship to its own ethnic and racial minorities.

This is a useful book that tackles some neglected topics in liberal theory. Spinner provides a sophisticated and sympathetic account of how liberal societies shape cultural identity. Some readers may find the normative arguments rather thin. As Spinner admits, he is more interested in providing a "reading" of North American society—identifying its practices and implicit ideals—than in reexamining liberal principles themselves for potential cultural biases. Yet this is still valuable, since most liberal discussions of nondiscrimination ignore the ambiguities and complexities involved. Unfortunately, the book is marred by Spinner's misinterpretations of other theorists, particularly those he calls *cultural pluralists*. According to Spinner, cultural pluralists are committed to the view that immigrant groups should "keep intact their culture from their old country" and that "all aspects of all cultures should be celebrated" (pp. 66, 177). Spinner argues that such views (which he attributes to Iris Young, Ali Mazrui and others) ignore the fact that cultures change over time and that some oppressive cultural practices are unworthy of celebration. This is a staggering misinterpretation of Young's views and her underlying theory of 'transformative' politics, which are is much more nuanced, focusing (like Spinner's) on the way particular historical practices and social institutions result in injustice to particular groups. Indeed, Spinner's defense of increased political representation as a way of overcoming the historical oppression of blacks is similar to Young's view, although Spinner does not acknowledge this. Spinner's critiques of other cultural pluralists rest on similar misinterpretations. It is surprising that such an able observer of

American society should be such an unreliable interpreter of other political theorists.

University of Ottawa WILL KYMLICKA

Signifying Woman: Culture and Chaos in Rousseau, Burke, and Mill. By Linda M. G. Zerilli. Ithaca: Cornell University Press, 1994. 214p. $32.50 cloth, $13.95 paper.

The moment Simone de Beauvoir asked that distressing little *what*-question, the concept of *woman* became a problem of primary significance in feminism. In feminist political theory, Linda Zerilli observes, "What is woman?" was and still remains a rhetorical query that actually carries a correct answer. Woman *is,* women *are.* Accordingly, contemporary feminist studies of Western political thought tend to assume a more or less fixed and stable referent ("women") in order to unmask how (canonical, masculinist) theorists repeatedly domesticate, marginalize, or banish an identifiable group of human beings from public life (p. 2). Zerilli appreciates the insights of these feminist projects; but if feminists want to interrogate political theory effectively, she argues, they must not begin with women. Rather, they must decode *woman*—the "cipher" that political theory repeatedly figures, signifies, deploys, and yearns to control (p. 2). Having issued this bracing challenge, Zerilli begins the decoding herself, by resignifying the woman question in Rousseau, Burke, and Mill. These resignifications not only advance beyond existing feminist projects; they mark the end of feminist critique—and perhaps the binary of gender—as we have known them in studies of political thought.

Zerilli's provocative woman is neither a referent nor something signified outside a text but a sign, a signifier, and a signification articulated within the "symbolic space of political meaning" that is the text (pp. 6, 139). Following closely the psycholinguistic theory of the French feminist Julia Kristeva, Zerilli posits the sign-signifier-signification *woman* not as a locus of stability but rather as wholly arbitrary, fundamentally unstable, and, at the same time, captivating to the political theorist as writing subject. Why the captivation? Zerilli answers with an account of political theory that gives Sheldon Wolin's "response to crisis" perspective a French twist. In the act of confronting the threat of social upheaval and political calamity, she suggests, the theorist experiences "abjection" or a "demarcating imperative" that carries with it a sense of its own futility and danger (p. 10). The *abject* (a term borrowed from Kristeva's account of maternal separation and the instability of the speaking subject) disturbs and renders chaotic precisely what the (paternal) text struggles culturally to contain—"identity, system, order" (p. 11). At this intersection of chaos and culture, the "abyss" always beckons, and the prospect of pure containment through linguistic signs can never be secured (pp. 8, 11, 15). But the captivated theorist will code the "manly" struggle with self, language, and politics nonetheless, Zerilli tells us; and he "will code it *woman*" (p. 15). Thus, woman captivates, but she also menaces—as the theorist frantically endeavors to fill her absence, organize her space, and harness her energies. (In the case of Zerilli's Burke, the thrust of these endeavors proves horrifying.) Meanwhile, the text and its attempts to signify hurtle into a "sociosymbolic bedlam" or "semiotic chaos" for which woman "becomes a marker and a scapegoat" (p. 8).

This account of the political theory tradition is bold, and it will probably not appeal to scholars who are unmoved by the disruptive interpretive strategies of deconstruction and psychoanalysis. So be it. More relevant to Zerilli's project will be the response of feminists who have only "the most minimal truck with Julia Kristeva" (in the terms of Nancy Fraser's advice). One problem at issue for feminism is Kristeva's structuralism, which leans heavily on an account of language as a self-reproducing symbolic system. Another is the claim that Kristeva's (Lacanian) psycholinguistics reproduces in the categories *semiotic/symbolic* what Judith Butler calls "the sanctioned heterosexuality" of a deeply structured gender binary that is universalized in forces that Freud originally codified. Zerilli recognizes the language problem and the "universalizing gesture" of Kristevan structuralism and psychoanalysis and tries to balance them with an appeal to Bakhtin and "historically variegated meanings" (pp. 9, 12). But her decisive invocation of of symbolic and semiotic modalities tips the balance in favor of the gesture, rather than against it. Thus, at its most "meta" level, her interpretive strategy is in danger of recapitulating at the bottom precisely what it disrupts in its resignifications on top—an ahistorical and symbolic binary that is coded *feminine/masculine.*

Nevertheless, what happens on top, in Zerilli's readings of Rousseau, Burke, and Mill, is imaginatively complex. This is because, in her actual interpretations, Zerilli's Kristevan impulses relax. Especially by the time we get to the British, the Kristevan psychosymbolism morphs into a dialogic Bakhtinian picture of Burke's "carnivalesque language" that in turn, becomes a sociohistorical, Foucauldian picture wherein Mill's "symbolic economy" is theorized in a context of regulation, exclusion, and discipline. Zerilli's readings are also compelling, quite apart from Kristevanism, because interpretatively she treats each theorist as an irreducible particular. As a result, she delivers three startlingly different accounts of how *woman* signifies in the multiple and contradictory meanings that inhabit the figurative language of Rousseau's, Burke's, and Mill's texts. Simultaneously, she shows us how the language of the text is awash in multiple figurations of the feminine and is thus only *apparently* binary in its encounter with gender and order.

One brief example, from Zerilli's horrified and horrifying Burke, will have to suffice. By posing woman as a referent, we can find in the *Reflections on the Revolution in France* the familiar and reassuring binary that Burke celebrates as the traditional world of *king:masculine/queen:feminine.* But once woman is resignified as a site of symbolic chaos and the "frenzied, cannibalistic mothers, harpies and furies of hell" of Burke's imagination are unleashed, the reassuring *feminine:queen* is engulfed in the political horror of a feminized Jacobin abomination and the "writerly horror" of an uncanny feminine sublime (p. 85). By destabilizing the symbolic language of Burke's text, Zerilli shows us (as a psychostructuralist theory cannot) the incoherence of any deep gender binary. Thus, she breaks free from the conceptual straitjacket of the fixed masculine/feminine that has constricted so much feminist thinking—and encourages feminists to do the same.

In many respects, *Signifying Woman* is a fitting, if more theatrical, companion to *The Signifying Monkey* (1988), Henry Louis Gate's methodical literary study of how another "curious figure" of signification operates in a tradition of interpretation and "luxuriate[s] in the chaos of ambiguity" (*Signifying Monkey*, pp. 44–45). But in one important respect, Zerilli and Gates diverge. In the "theory of a tradition" that Gates develops, the black language of "Signifyin(g)" is a discourse, an emancipatory form of verbal art. In Zerilli's reading, the political theory tradition emerges as a symbolic (if chaotic) order bent on order; its "signifying" offers no equivalent possibilities for the coordination of action or the constitution of a liberatory discourse. Within this order, feminist theory can destabilize and defamiliarize the imperatives of signifying woman, but it cannot construct a programmatic alternative out of them. To leave feminist theory here is to leave much unsaid, I think, about the emancipatory potential in the activity of text interpretation, not to mention the transformative potential of theorizing as a discursive political activity. But Zerilli has given us much else to contemplate, and that is enough for now.

University of Minnesota MARY G. DIETZ

AMERICAN POLITICS

A Woman's Place is in the House: Campaigning for Congress in the Feminist Era. By Barbara C. Burrell. Ann Arbor: University of Michigan Press, 1994. 211p. $37.50.

Barbara Burrell's book begins with the familiar recognition that women are substantially underrepresented as a group in the U.S. Congress.

Twenty-five years into the contemporary feminist era, women held only 6.6% of the seats in the Congress, the fourth lowest proportion among the 25 mostly Western democracies she poses as a comparison group. Even after the substantial gains by women in the 1992 election, the proportion of women in the American national legislature rose to only 11%, still lower than a majority of the comparison group. Burrell's goal is to explain this underrepresentation by focusing on the experiences of the women who have run for the House of Representatives since 1968, the election she notes as the first in this new era of political feminism. The strength of this book is that it is the first to provide longitudinal data examining this fascinating question. While others have explored some aspects of this seeming anomaly or have looked at women candidates in one election, set of elections, or geographic area, Burrell has gathered data on virtually every aspect of the congressional electoral process as it has been experienced by female candidates from 1968 through 1992.

The method employed by Burrell is straightforward. In each of the first six substantive chapters, she presents theoretical material on how female candidates might be impinged on differently from male candidates by a particular aspect of the electoral process. She then derives empirically testable hypotheses from the theories reviewed and presents the data to test those hypotheses. For example, an entire body of literature explores how citizens view political leaders. If citizens attribute characteristics to women different from those normally associated with political leaders, it would follow that women would be disadvantaged, because of gender, as citizens determined whom to vote for. Burrell looks at polling data and social science experimentation over the last decades and rejects citizen attitudes toward women as a cause of the disproportionate underrepresentation of women in the Congress.

The conclusion Burrell draws from this review might surprise the casual observer. Essentially, she rejects hypothesis after hypothesis holding that the process is prejudicial toward women: the public does not reject female characteristics as inappropriate for political leaders; women do not do disproportionately worse than men in primary elections; the background characteristics of female candidates are not substantially different from those of male candidates; party leaders, in those states in which their influence still matters, do not discriminate against women (or slate them only for hopeless races); women can and do raise as much money as male candidates, and they do so at the strategic times in races from similar sources; female candidates do not fare worse than their male counterparts (all other things being equal) in general elections. Why, then, the disproportionate representation of women in the Congress? Burrell poses two reasons. First, all candidates, male and female, are disadvantaged when they run against incumbents; until very recently, overwhelming numbers of incumbents have sought reelection and overwhelming numbers of them are male. Second, women candidates do not run as often as do male candidates. She begins to explore some of the reasons why this is so, but one of the few disappointments in this book is that her data and that exploration stop short of presenting a convincing explanation of this cause.

Burrell's last substantive chapter deals with women and policy representation. In some ways, that chapter should have been first, to set the context of why this study is important; but the importance of the questions stands, without this seemingly unrelated and not overly informative chapter.

In 1992, "the year of the woman" in congressional elections, the number of women in the House doubled. One cannot help but wonder if in its original design this study was intended to end with the 1990 election, before the decennial reapportionment and redistricting but was extended because of the striking gains by female candidates in 1992. If that was the case, the extension was wise and added to the quality of the book by demonstrating how this election, which appeared to be an outlier for women, in fact confirms Burrell's findings. More candidates did run, in contexts that enhanced their chances, and they won.

That experience confirms that Barbara Burrell has produced a book that makes an important contribution to our understanding of the electoral process and of the role of women in that process.

Colby College L. SANDY MAISEL

Behind the Mule: Race and Class in African-American Politics. By Michael C. Dawson. Princeton University Press, 1994. 234p. $24.95.

This book extends the study of African-American politics in several significant directions. Dawson provides an innovative theoretical framework to generate testable propositions about the relative contributions of class and race as heuristics by which African-Americans understand politics and public policy. The empirical case is made based on historical, aggregate, and survey data. Most notable is the National Black Election Study (NBES), which has proved to be an important contribution to researchers interested in public opinion. Dawson's conclusions will surely generate serious discussion of the current and future state of African-American politics. In short, Dawson's original analytics and wide-reaching empirical tests mark *Behind the Mule* as a new standard in the field of African-American politics.

Dawson starts with William Julius Wilson's finding that the black class structure is bifurcating as a result of the restructuring of the American economy in the last quarter of the Twentieth century. The main focus of this analysis (chap. 2) concerns the degree and stability of economic polarization among African-Americans. Dawson utilizes aggregate data to show the persistence of large black-white differences in income, unemployment, labor market participation and youth unemployment. Further, he describes how government policy has supported racial inequities in economic status. Although Dawson concurs with Wilson's basic finding about black economic polarization, he is less certain about the stability of the gap. In particular, Dawson is sensitive to the fact that the black middle class is vulnerable: they have less wealth relative to their white counterparts and are disproportionately employed in the public sector. Nonetheless, his conclusion is that significant variation in economic status, both inter- and intraracially, raises the possibility of class-based political differences.

Dawson correctly notes that the literature does not support the hypothesis that differences in political behavior and predispositions are simply a function of differences in socioeconomic status. In other words, why do middle-class blacks appear to perceive their individual interests as highly correlated with black group interests? In chapters 3 and 4, Dawson unpacks a powerful analytic account of the nature of group interests and how these interests serve as a heuristic by which African-Americans understand politics and public policy. Relying on concepts from rational choice theory, Dawson introduces the *black utility heuristic* to account for "the way individual blacks go about evaluating policies, parties, and candidates" (p. 57). In other words, when the "black experience" (i.e., the cost of being black in America) is uniformly distributed across the black populace, it is rational for group cues to guide individual political assessments. Dawson argues that this is likely for several reasons. First, researchers such as Tajfel and Turner have shown the saliency of group membership in the development of social identities. These identities, in turn, aid in the evaluation of in-group and out-group interests. Second, membership in black social networks and institutions reinforce group identity. For example, membership in churches and fraternal organizations provide structural opportunities for individual interests to be discussed and mobilized for group goals. Finally, the historic conflict between blacks as a subordinate group and whites as the dominant group confirms the rationality of group-based calculations. From this view, the economic and social legacy of slavery produces a strong unifying experience that supports the use of group interests as a cue for the identification of individual interests. Cumulatively, these reasons lead Dawson to conclude, "The historical experiences of African-Americans have resulted in a situation in which group interests have served as a useful proxy for self-interest" (p. 77).

Dawson identifies two critical elements of group interest: a sense of *linked fate* and evaluations of relative group economic status. Utilizing NBES data, Dawson reports several notable findings. First, perceptions of linked fate and black economic subordination are weakly correlated with measures of social-class position. Moreover, the only class effect reported was in the opposite of the expected direction: the higher the level of education, the more likely one was to believe blacks are economically subordinated. Second, a causal model of group interests supports the conclusion that the two dimensions of group interest serve mutually to reinforce one another; that is, blacks who believe the group is economically worse off compared to whites are also likely to have higher levels of some of linked fate. On the other hand, blacks who perceive a high level of linked fate are less likely to believe that blacks are better off than whites. Third, the impact of class is stronger than the impact of linked fate for assessing the payoffs of the Civil Rights movement. For example, highly educated blacks perceive greater personal benefits from the Civil Rights movement than less educated blacks. In sum, these findings lead Dawson to conclude, "These models show that socioeconomic status only weakly influences perceptions of linked fate. The economic and political components of group interest have a major role in shaping perceptions of both economic well-being and relative group interest" (p. 88).

Chapters 5 and 6 empirically demonstrate how group interest serves as a heuristic to evaluate political parties and candidates. Dawson gives a historical account of the *derapage* of black support for the "party of Lincoln" and the unifying of black support for the Democrats. This analysis sets the stage for more rigorous tests concerning the causes of black support for the contemporary political parties. In the main, Dawson finds that perceptions of racial group interest serves as an efficient heuristic for evaluations of the political parties. For example, he reports that the economic dimension of black group interest has a significant effect on support for the Democrats and evaluations of which party best serves black interests. On the other hand, class divisions emerge on the question of a black independent political party: the less educated and affluent are more supportive of a black third party.

Chapter 6 extends the analysis by examining how perceptions of group interest structure African-American electoral choice. Here, group and class interests compete to explain support for Jesse Jackson and the presidential vote in 1984 and 1988. Dawson expresses his findings regarding Jackson in the following way: "Those who believed that their own fate was linked to that of the race, who were concerned about the dominant position of whites, and who believed that African-Americans should form their own political party saw strong possibilities in Jackson's candidacy" (p. 146). In terms of the presidential vote, the data suggest that class-status variables were weakly related to vote choice in both 1984 and 1988. On the other hand, group interest variables had a measurable effect. For example, those who believe African-Americans are worse off than whites are 14% more likely to vote for Mondale in 1984.

The findings in chapters 5 and 6 lead Dawson rightly to conclude that group interests dominate class interests in terms of partisan identification and national-level vote choice. To the extent that class differences are present, they run in the opposite direction predicted by class proponents. Nonetheless, Dawson is sensitive to the fact that focusing on party politics and presidential voting may mask as much as it reveals. Given this, it is puzzling that the next step in the analysis is to consider Gallup trend data on black presidential approval rates (chap. 7). Although the analysis is interesting in and of itself, it is weakened by the fact that the Gallup data allow for measurement only of the economic dimension of group interests. Further, it is unclear whether class interests are accurately reflected in the analysis. As a result, it is difficult to assess how much credence one should give to the finding that blacks weigh economic conditions more heavily than do whites in evaluating presidential performance.

Dawson returns to more central tests of his argument in chapter 8. In particular, he analyzes the degree to which race and class serve as heuristics with which to evaluate public policy. The measurement models focus on three dimensions of policy preferences: economic redistribution (government guarantee of jobs and income, spending on unemployment compensation, Medicare, and food stamps), racial policy (race-specific governmental policies, e.g., affirmative action and improving black schools), and black autonomy (giving children African names, always voting for black candidates, having nothing to do with whites, and forming a black third party). The basic pattern to the findings is that class divisions play an important role in informing individual blacks' policy views. This is particularly true for questions of economic redistribution and black autonomy. More educated and affluent blacks are less supportive of the basic redistributive policies that have traditionally formed the liberal agenda and less likely to support nationalistic views. On the other hand, linked fate dominated the model for racial policies.

It is plausible to read the evidence presented in chapters 4–7 as strong support for the group interest model. These data suggest that racial group interest serves a proxy for individual self-interest in determining evaluations of political parties and electoral choices. This is an important construction, given the centrality of partisanship to theoretical accounts of political attitudes and behavior. For the sake of argument, however, one might imagine a very different interpretation of Dawson's findings. Such a case might be defended on two points. First, some might contend that Dawson has not given the class model its empirical due. On this line of reasoning, relying on Wilson's conception of class prevents Dawson from exploring a measure more richly reflecting the subtleties of contemporary black class structure. For example, simple measures such as job authority, public- or private-sector employment, and job satisfaction might tease out some within-group variation in partisan affiliation and political behavior. Further, measures that seek to incorporate residence with class may prove fruitful. Orfield's work on Atlanta demonstrates how class status and place of residence influenced perceptions of interest and thus political views.

Another way to think about the evidence on policy views is that there are serious signs of a breakdown of group consensus on issues that most clearly define the debate about race in American society; that is, the splintering of black views, particularly on questions of economic redistribution, has highly charged implications for racial politics. From this perspective, Dawson's own analysis of the partisan realignment of black interests in the first half of this century is telling. One wonders if Dawson's findings are the harbinger of a similar contemporary occurrence. The class-based split in black views over economic redistribution is occurring at the same time as a Republican congressional landslide, the continued ambivalence toward blacks by Democrats (e.g., their treatment of Jesse Jackson, Sister Souljah, Lani Guinier), a third generation of black urban leadership (Schmoke, Archer, Webb, Rice), and the emergence of visible black Republicans at the congressional level (J. C. Watts, Gary Franks). These developments are all the more interesting in light of his finding that middle-class blacks are less interested in racial autonomy than lower-class blacks. By this account, group interest may weaken as the primary basis for individual political evaluations. For example, we might expect group interest to be less important for issues such as welfare reform, immigration, and crime. Further, the inclusion of someone like Colin Powell on the Republican presidential ticket might activate widespread partisan defection among blacks.

In sum, Dawson has built an original theoretical framework, subjected it to a variety of empirical tests, and reported some highly interesting findings. While some might challenge his interpretation of the effects of social class, *Behind the Mule* will surely be the basis for a new research agenda on African-American politics.

University of California, Los Angeles FRANKLIN D. GILLIAM, JR.

The State and Labor in Modern America. By Melvyn Dubofsky. Chapel Hill: University of North Carolina Press, 1994. 322p. $34.95 cloth, $14.95 paper.

A Muted Fury: Populists, Progressives, and Labor Unions Confront the Courts, 1890–1937. By William G. Ross. Princeton: Princeton University Press, 1993. 368p. $39.50.

The State and Labor in Modern America, by Melvyn Dubofsky, is an important contribution to the growing literature examining the foundations of the contemporary American state. In this work, he examines the interaction between the state and organized labor from the Gilded Age through 1973. He is particularly concerned with the way trade union growth and decline intersects with the persistent dominance of capital within a state context marked by "gross material inequalities within a regime of universal citizenship and mass democratic voting rights" (pp. xi–xii).

Those interested in the history of labor unions will find Dubofsky's book indispensable. In well-written and lucid prose, he charts key labor activities and goals that produce "a constitutional revolution" redefining the powers of the state and the federal government (e.g., the Wagner Act). He also captures the paradox—not to say *dilemma*—faced by labor leaders seeking accommodation with state forces. Samuel Gompers' success in eliminating the International Workers of the World union, for example, at times contributed to, rather than alleviated, antilabor orientations, thereby inhibiting, rather than catalyzing, the labor agenda defined more broadly.

Despite the extraordinary scope and complexity of Dubofsky's splendid work, some will be disappointed by its failure to situate itself within frameworks vital to an analysis of political reform, such as the impact of sectionalism and alliances between rural and labor interests. His view of the labor movement as coterminous with the disappearance of agricultural and rural sectors of society is at odds with other prominent scholarship (e.g., Richard Bensel and Elizabeth Sanders). In addition, he fails to integrate consideration of the primacy of labor as a repository for an antiliberal, antireform component of the American political heritage as so brilliantly illuminated in the work of Karen Orren.

Dubofsky also fails to situate his research within other contexts increasingly important to those concerned with the development of the modern American state, such as the relationship between state-level policies and national policies. Although he argues that the state level may be the most important political unit prior to the New Deal (p. xvii), he nevertheless ignores grassroots constituents as a potential resource for nationalizing public policies at the federal level after the turn of the century. He also omits attention to the way the labor story does—or does not—adequately deal with gender issues. He sees trade union interests emanating from class, in distinction to race and gender issues (driven by civil rights concerns), thereby obliterating attention to common denominators operating across race, class, and gender concerns. These reservations aside, however, Dubofsky's work is an important contribution to the field of labor studies in particular and history and politics more generally.

A Muted Fury, by William G. Ross, is a magnificent study and a must-read acquisition for anyone interested in the way reformers in the Progressive Era used judicial power in the United States to develop labor policies, as well as the capacity of the legislative process at the state and national levels to redefine the role of government as a tool for regulating relationships between employers and employees. Ross's book explores the expansion of state power by asking the question, What is "the proper scope of judicial power" in the American political system? (p. 1). A subtheme of his book is the assertion that "the judiciary can never be apolitical" (p. 3).

Although it has been usual to think that the courts in the Progressive Era were staunch defenders of "a rigid doctrine of laissez faire," Ross's work shows how this is now giving way to "increasing recognition that the courts were remarkably amenable to Progressive reforms" (p. 16). Judicial review may be a distinctly American innovation, but it is not a power that courts wield in an absolutely autonomous way. As Ross shows, agitation by Progressive reformers against the courts effectively persuaded many judges to uphold Progressive legislation they might otherwise have invalidated (p. 315).

Ross notes that the Progressive Era is a time period in which reformers attempted to forge new public policies to deal with the social, economic, and political dislocations accompanying rapid industrialization. Yet Progressive reformers, far from representing the common people, often internalized many contradictions and ambivalent attitudes toward the development of a stronger state. Using an unusually wide range of research materials (e.g., newspapers, journals of the time period, and a careful reading of the court cases at issue), he shows how the regulatory state that comes into being embodies conflicts about how to combine the scope of the state's police power with notions of public interest, not to mention the inherent tensions involved in transfers of power from the state to the national level in a federal system.

Ross's book is very much in tune with the "new institutionalism" integrating history and politics along the axes of institutional and structural analyses. Yet attention to the work of those scholars most responsible for kicking off this intellectual enterprise (e.g., Skowronek, Skocpol, Orren, Bensel, Sanders, Heclo, Shefter, Burnham, and Sklar) are all but absent from his work. This is a shame, because such omission robs his book of an opportunity for integration with other major interpretations of American state development. This one reservation aside, however, Ross's book will be of great value to everyone interested in the Progressive Era and the development of the modern American state.

Northeastern University EILEEN L. MCDONAGH

New Deals: Business, Labor, and Politics in America. 1920–1935. By Colin Gordon. New York: Cambridge University Press, 1994. 329p. $59.95 cloth, $17.95 paper.

In this work, Colin Gordon examines New Deal regulatory and welfare policies in the context of antecedent problems of class organization. He begins with an analysis of the Trade Association and Trade Union movements in the 1920s, exploring the dilemmas of collective action among competitors the better to understand the limits of associationalism and examining how trade unionism can provide stability and relief from competition for businesses. Gordon then turns to studies of the National Recovery Administration, the Wagner Act, and the Social Security Act to understand these regulatory and welfare programs of the 1930s as a response to the limits of voluntary action in the 1920s.

Gordon describes the New Deal as a response to dominant business interests faced with an unprecedented economic challenge. The Trade Association and Union movements of the 1920s, which had been the means for rationalizing the market preferred by business, proved insufficient to deal with the competitive pressures of the 1920s and utterly incapable of confronting the distresses of the 1930s. Under these circumstances, business embraced the regulatory and welfare state during the New Deal. When that state proved to be more costly and inefficient than dominant business interests anticipated, they became disenchanted with New Deal solutions and attacked their own creations. The power of dominant business interests notwithstanding, an institutionalized New Deal proved capable of resisting the newfound hostility of the business community, especially since the advent of World War II had compelled partisans to lay aside their differences and led to improved economic condition for business.

The thesis is a variation of familiar themes. The argument that the New Deal was the product of dominant business interests has been a staple of radical American historiography for many years. Similarly, the claims that the state is relatively autonomous, that the administrative capacities and incapacities of the state play an important role in explaining the success or failure of regulatory programs, and that conflicts between dominant business interests and state actors reflects divergent institutional perspectives but not fundamentally incompatible interests are not new. This book develops and synthesizes wealth of historical information to provide a richer empirical foundation for familiar themes.

Gordon is more innovative in his only partially developed suggestion that business openness to national economic planning and the extensive regulation that it entailed was an immediate response to the Depression, rather than a response to the enduring dynamics of advanced capitalism. If the Depression did not simply accelerate a trend toward growing business appreciation for the role that government could play in bringing stability to markets (i.e., if the Depression was an atypical period of economic chaos that disoriented business and led businessmen to support regulatory solutions that they would not have considered in times of normalcy), then it becomes more understandable why businessmen turned against the New Deal at the first signs of economic recovery. Gordon underestimates the depths of business hostility to the New Deal and fails to understand how early in the New Deal it became significant, but his focus on the eventual emergence of business opposition does provide insights that at times transcend his theoretical framework. While his theory emphasizes the inefficiencies of the New Deal, his historical account provides examples of business concern with a loss of managerial prerogatives or power. Business was in fact concerned more with a loss of control over the agenda and ends of regulation than over the inefficient implementation of regulation.

The fundamental weakness of this book is that fails seriously to engage alternative interpretations of the historical record that it so richly describes. The dominance of business interests is not proved, it is simply asserted as the presupposition of all further interpretation. Gordon preaches to the converted. The sermon is powerful, but it does not win converts.

Wilkes University DONALD R. BRAND

Prisoners of Myth: The Leadership of the Tennessee Valley Authority, 1933–1990. By Erwin C. Hargrove. Princeton: Princeton University Press, 1994. 374p. $45.00.

In *Prisoners of Myth,* Erwin Hargrove probes with comprehensiveness, insight, and cogency of argument the organizational evolution and decline of the Tennessee Valley Authority (TVA). His tale, however, is more than a finely textured, revealing, and definitive history of the TVA. Hargrove masterfully uses the TVA experience to demonstrate and explore an uncommon theoretical insight: the sources of a "historically successful" organization's failures and decline may lie, quite ironically, in its prior successes.

Hargrove's research on the evolution of the TVA's organizational processes, culture, leadership, and political environment is encyclopedic, adroitly integrated, and skillfully recounted. He examines, in novel ways, how the actions and disparate philosophies of the founding generation of TVA board members (A. E. Morgan, Harcourt Morgan, and David Lilienthal) created an institutional culture that simultaneously informed and confounded the purposes of subsequent TVA leaders. The "myths" informing this culture included the idea that the TVA was a multipurpose resource development agency, not a power company; that the judgment of TVA's professional work force should—and could—always trump political considerations; that decision-making power should be decentralized within the TVA in its separate programs; and that TVA's production of low-cost electric power would always create its own demand.

Prisoners painstakingly recounts how each of these heroic "myths" immeasurably tarnished the TVA's image in the 1970s and 1980s. During those years, Hargrove alleges, TVA board chairmen Aubrey J. Wagner and S. David Freeman mistakenly attempted to recapture past TVA glories by building the Tellico Dam and by trying to develop the nation's largest nuclear power program. Both projects tried to recapture the TVA's early organizational elan, to reintegrate a disastrously balkanized and bureaucratically driven organization, and to reposition the TVA as a national leader. They failed, Hargrove insists, because Wagner and Freeman's purposes were ill suited to the political environment of their times, their own personal skills were inadequate for realizing these purposes, and they were insufficiently strategic in altering the TVA's existing culture.

Hargrove then derives a theory of organizational failure by deftly melding his findings with prior research on leadership, organizational change, technological complexity, and public accountability. He proposes, for example, that leadership tasks and requisite skills will vary across different stages of an organization's life. Leaders at the "founding" must create and sell "meaning" both inside and outside the organization, the next generation has to institutionalize that "myth," and their successors must recast original missions when these are realized, become less politically viable, or lose their relevancy. The choice is to either repackage the heroic myth or to scale it back to less heroic dimensions. Most leaders, however, instinctively take the heroic route and set their organizations on a course of folly. They do so by inaccurately assuming that the skills, organizational processes, and political strategies that produced past successes are readily transferable to new missions—pursued in new environments—with greater levels of task complexity.

While its virtues are considerable, *Prisoners* may also disappoint readers in certain respects. First, Hargrove clearly implies that Wagner and Freeman were mistaken in undertaking the "heroic" approaches that they did. As he notes, however, not only was the Reagan administration's embrace of nuclear power well known, but the TVA's political environment in the valley and on Capital Hill would have made pursuit of anything less quite problematic. Thus, playing the nuclear card to trump Reagan's well-known aversion to the TVA could be considered a brilliant attempt to match purpose with politics. Successful leadership may thus lie more or less in the eye of the beholder or may be divined only in retrospect. Neither trait bodes well for theory building.

Neither does Hargrove offer criteria for deciding a priori what leaders should do under different circumstances. For example, while he points out that two strategies—myth fulfillment and myth retrenchment—are available and may be appropriate, he fails to discuss the conditions under which one or the other is more suitable. Likewise, while he faults the TVA's historical tendency to avoid centralized planning and to foster decentralized decision making, Hargrove offers little in the way of advancing our thinking about the conditions under which these strategies can be more or less effective in the hands of leaders attempting to match purpose, politics, and processes. And while *Prisoners* is a virtual catalogue of well-documented bureaupathologies (e.g., bureaucratic pluralism, professional antagonisms, and turf wars), the reader gleans little about how leaders can successfully deal with these issues more generally.

Finally, one of Hargrove's major arguments needs buttressing. He posits that failures in historically successful organizations are due primarily to heroic efforts at innovation, rather than bureaucratic "sluggishness or routinization after the founding." TVA's evolution, however, suggests that *both* innovation and organizational sluggishness interacted to imperil the agency. Sorting out the relative contributions of each to the TVA's woes seems to me problematic. Still, *Prisoners of Myth* is a valuable addition to the literature on leadership in public organizations. Hargrove's theses about organizational failure are important and merit testing, elaboration, and refinement in future research. What is more, students of the TVA, of leadership in public organizations, of bureaucratic politics and accountability, and of organizational dynamics will find his research, methodology, and insights rewarding reading.

University of Baltimore Robert F. Durant

Legislative Leadership in the American States. By Malcolm E. Jewell and Marcia Lynn Whicker. Ann Arbor: University of Michigan Press, 1994. 232p. $39.50.

Utilizing prior studies in management science, presidential studies and congressional leadership, Jewell and Whicker provide their readers with an extremely extensive analysis of the context, nature, styles, and techniques of legislative leadership, as well as a conceptual scheme for studying that leadership. Although the data and examples cited focus on leadership in contemporary American states, their conceptualization and operationalization of various leadership aspects provide insights useful in *any* political setting. This will become must reading for any student of legislative leadership.

Three major contributions to the literature on legislative leadership are apparent in this book. First, there is a proposed conceptual approach to measuring leadership aspects using a rich theoretical base. Five major dimensions are identified: historical national and state trends, institutional structure, leadership's partisan position, leadership tools and techniques, and personal leadership type (see tbl. 1.1 and app. 2). Abundant illustrations are provided regarding the composition and effects of each dimension, including their components and specific indicators; relationships and possible hypotheses are also explored. The utility of this approach is exemplified by a typology based on leadership style (command, coordinating, and consensus) and leadership goals (power, policy, and process) proposed to analyze state legislative leadership behavior (chap. 6).

A second contribution is the rich set of examples, detailed for the reader, illustrating the application of this scheme to specific American state legislative settings. Based on interviews with approximately 90 state legislative leaders in 22 states, the authors provide a wealth of information about the leadership activities, predispositions, and styles in actual legislative settings. This does *not* become a routine "laundry list" of leadership practices in state legislatures but, rather, a rich exposition of state legislative leadership behavior demonstrating how leadership works. The attention provided to themes like the increasing independence of individual legislators, the growth of professionalism among members, the decreasing power held by most legislative leaders, the erosion of party unity, the advent of polarization within and between legislative parties, and the implications of term limits on leadership and legislative operations enhances one's appreciation of the context within which contemporary legislative leaders function. Anyone interested in American state legislative leadership will find extensive information and will learn from the authors' insights regarding how these legislative leaders work. Jewell and Whicker communicate their expertise on state legislative leadership very effectively through their integrating examples of leadership behavior with the general principles of leadership style.

The third major contribution of *Legislative Leadership in the American State* is its well-documented and thorough treatment of the increasing numbers of women legislative leaders. This too frequently neglected topic of gender and legislative leadership appears both as a theme throughout the book and as a topic for more in-depth treatment in a separate chapter. The authors conclude that women legislative leaders are less likely than their male counterparts to adopt leadership types associated with a "command-power" approach.

The specification of a systematic measurement scheme for studying legislative leadership is the most significant contribution of *Legislative Leadership in the American States.* Subsequent researchers can scrutinize this approach and experiment with applying it to various legislative settings. No doubt, changes will occur and new indicators will be added; yet the authors have provided us with a critical "road map" to aid in the further study of leadership in any legislative organization.

While reference is made throughout the book to hypotheses suggested by the data presented, there is no testing of factors affecting leadership style and goal adoption or of the consequences of leadership style and goal adoption on legislative activities, member behavior, or policy outputs. Such testing is left to the subsequent work of others who can build on the insights, relationships, and theory testing suggested herein. One shortcoming, conceded at the outset is the absence of information, perceptions, and perspectives from rank-and-file legislators. Data collection from "followers" is a standard measurement strategy found throughout management science and organizational theory studies of leadership. Only as one is able to bring these perceptions into the analysis of leadership does one have a balanced and objective perspective. While Jewell and Whicker agree explicitly with this necessity, it is left largely to subsequent researchers to determine how to integrate this measurement piece into the study of legislative leadership.

University of Rhode Island Ronald D. Hedlund

The Losing Parties: Out-Party National Committees, 1956–1993. By Philip A. Klinkner. New Haven: Yale University Press, 1994. 238p. $30.00.

By common consent, any list of effective national party committee chairs would include Democrats Paul Butler and Ron Brown and Republicans Ray Bliss and Bill Brock. Apart from the letter *B*, the common thread among these chairmen is that they all served when their party did not hold the presidency.

In this excellent book, Philip A. Klinkner helps us understand this connection. Out-parties, he explains, differ from in-parties. The national committee of an in-party usually defers to the president and the White House staff, with its role falling somewhere between subordination and servility. And in the flush of a presidential election victory, the in-party national committee usually has little taste for changing any of its ways. Defeat cures a national committee's complacency while increasing its autonomy. As Klinkner says, "losing parties are more likely than winning parties to innovate and reform themselves and the party system and to raise new issues" (p. 1). So to understand the dynamics of the national party organizations, we must pay special attention to the losers.

Drawing on party documents, press accounts, and interviews with nearly one hundred party figures, Klinkner skillfully tells how each out-party since 1956 has responded to losing the previous presidential election. These responses, he says, fall into three categories: policy-oriented (statements on public issues), organizational (finance and management), and procedural (internal governance and the processes for choosing presidential candidates).

The clearest instance of a policy response, Klinkner shows, came during the second Eisenhower administration. Led by chairman Paul Butler, the Democrats set up an Advisory Council to lay out a policy agenda and give a well-publicized sounding board to liberals who were frustrated with conservative committee chairs in Congress. After the 1968 and 1972 elections, by contrast, Democrats put more emphasis on procedural responses, namely, the reforms of the nominating process that profoundly changed presidential campaign politics.

The book's strongest chapter focuses on the GOP's organizational response after President Ford's 1976 defeat, when Chairman Bill Brock vastly enlarged party efforts on fundraising, communication, training, and use of technology. Many observers have likened Brock to Ray Bliss, who helped rebuild the party's infrastructure after the Goldwater defeat of 1964. Klinkner says that Bliss embodies the "nuts-and-bolts" style that most party operatives endorse: "In the pantheon of party gods, Ray Bliss is the Republicans' Zeus" (p. 258n.). Klinkner's key insight is that the similarities between Bliss and Brock have eclipsed crucial distinctions. Whereas Bliss wanted mainly to strengthen state and local parties, Brock strove to turn the Republican National Committee into the center of party activity. Klinkner makes clear that the recent "nationalization" of party politics owes much to Brock's tenure.

He concludes by explaining how each party's unique culture shapes its response to out-party status. The Republicans, as the party of business, tend to see campaign politics in organizational terms; and their national committee operatives regard defeat as a problem in management and marketing. The Democrats, as the party of interest and constituency groups, focus on procedural responses intended to ensure a voice for all their diverse elements.

The Losing Parties represents an impressive feat of research. With their haphazard document storage and sometimes-paranoid staffs, national party headquarters are often user-hostile for academics; nevertheless, Klinkner has managed to wring fresh information out of them. He has also filled every page with crisp, clear English. While not stinting on theoretical concerns, this book will make enjoyable and edifying reading for undergraduates and political practitioners. For scholars, it provides a valuable reference that combines a detailed account of internal party struggles with a broad analysis of the political setting. The GOP chapters will be especially useful, because political science has tended to overlook Republicans.

This book supplies a starting point for further work. Klinkner observes that Ron Brown, Democratic chairman during the Bush years, gained greater media visibility as a party spokesperson than any previous national chair. During the first half of the Clinton administration, Republican chairman Haley Barbour had a comparably high profile as a fixture on television talk shows. Party scholars should examine the rapidly changing role of the national party chair in the new age of "punditocracy."

In addition, they should keep an eye on how the professionalization of politics will affect party cultures. Until very recently, party operatives learned their craft in the proverbial school of hard knocks. Today, they are gaining at least part of their political education in places such as the Graduate School of Political Management, where Republicans and Democrats sit side-by-side and learn the same lessons about organization, strategy, and tactics. It is plausible that this shared experience could eventually narrow some of the cultural differences that Klinkner so ably describes.

Claremont McKenna College JOHN J. PITNEY, JR.

Strategic Budgeting. By Roy T. Meyers. Ann Arbor: University of Michigan Press, 1994. 238p. $42.50.

Incrementalism as a budgetary explanation has been attacked for many years on many grounds. *Strategic Budgeting* provides additional grounds for questioning the value of the incrementalistic interpretation. Meyers uses his experience in the Congressional Budget Office, archival sources, and interviews to explore strategies based on budget structure. Rather than rely on mathematical characterizations of the federal budget process, he describes how budgeting and structure affect each other. He argues that strategies of budget participants are constrained by the structures within which they work. Moreover, the participants, be they budget advocates or budget controllers, act to create structures that yield the budget outcomes they desire. This book insightfully delineates the interaction of budget strategy and budget structure.

While Meyers's concept of budget structure is admittedly not crystal clear, there is more than enough clarity that the reader can understand the argument. At the risk of oversimplifying Meyers's argument, the logic of the book is that if you change the rules of the game, you should change the outcomes. He then delineates structures and strategies and shows some of the attempts to change rules that have had some success. He adds to the catalog of ploys that budget participants use to advance their causes. These rules or behavioral regularities are more complex than an incrementalist would like. Their presentation does not promise much predictive value for the student of federal budgeting. But Meyers does make some rather compelling interpretations of what goes on in the budget process. Even though there may not be great theoretical elegance in his presentation, the raw material he serves up is very valuable.

Meyers is best when he works at the detailed level of what actually happens from the perspective of the budget participants. Glorying in some accounting details, for instance, he demonstrates that the seeming clarity and immutability of the budget numbers are often chimerical. Because much of the political science budget literature is based on data found in the fog bank that characterizes some budget figures, Meyers's analysis raises questions about much that is found in the academic budgeting and policy literatures.

Meyers shows how accounting techniques or tactics can advantage program advocates. Establishing off-budget status, spending through tax expenditures, creating private government enterprises, paying with cash equivalents, underestimating payouts, leasing, and avoiding full funding requirements are some of the accounting tactics he describes to show how the structure of accounting for money can affect budgets and funding streams.

Similarly, he shows that underfunding, playing with the baselines, selective collaboration and isolation, creative use of inflation, and other budget decision procedures help make decisions that change how the game is played. The ability to dedicate funds, capitalize high support levels, diversify financing sources, index spending, and maximize administrative discretion are shown to create favorable outcomes. Meyer also presents numerous rationales for the spending of money that help program advocates continue to prosper. In effect, his treatment of rationales walks the reader through a handbook of budgeting rhetoric.

Some may find the book a bit journalistic in style. He tells stories, he makes arguments, he prescribes. In some parts, he seems to be saying, "trust me," because his methodology is dependent on evidence and testimony from sources and persons whose identity he cannot reveal. Yet the preponderant weight of evidence he presents is available from the public domain and so should be credited. Meyers adds one more series of data to the discussion on why the academic incremental explanations are lacking. At the *details* level of providing information, the book makes signal contributions. As a theory or embracing perspective, it could be tighter, but it helps us understand what is really happening in many arenas of federal budgeting.

Although the book begins as an attack on the received wisdom of incrementalism, it ends as a series of policy prescriptions. Meyers wants to see budget reform and finishes with a rather skeletal series of recommendations.

University of Illinois, Chicago JOHN WANAT

Icons of Democracy: American Leaders as Heroes, Aristocrats, Dissenters, and Democrats. By Bruce Miroff. New York: Basic Books, 1993. 422p. $25.00.

Is there such a thing as "democratic leadership," or are the two terms (in Thomas E. Cronin's words) "warring concepts"? In this ambitious, and thought-provoking work, Bruce Miroff examines "the rich variety of forms that American political leadership has taken" (p. 2) and explores the possibility of achieving forms of democratic leadership in the United States.

Recognizing that "leadership has rarely fit comfortably with democracy in America," and that "the most committed democrats have been suspicious of the very idea of leadership," Miroff asks, "What kinds of democratic leadership and followership might be possible in America?" (pp. viii, 1). Is the tension between leadership and democracy creative or destructive of democratic values?

Miroff finds four types of leadership in the United States: *aristocratic, democratic, heroic,* and *dissenting*. The author then examines each of the styles of leadership as practiced by representative figures in American history. Aristocratic leadership attempts "to tame the democratic passions of the American masses" by allowing those of "superior eminence" to rule over the people (p. 5). The two different but related examples of aristocratic leadership discussed by Miroff are Alexander Hamilton and John Adams. This leadership style, according to the author, is not compatible with democracy. Democratic leadership "requires a respect for followers, rooted in a recognition of what Herman Melville called the 'democratic dignity' of every individual" (p. 2). The goals of democratic leadership are egalitarian. Abraham Lincoln and Franklin D. Roosevelt serve as Miroff's icons of democratic leadership, with Lincoln serving as "the best model Americans have of democratic leadership" (p. 124). Dissenting leadership tends to come, understandably, from outsiders. They challenge the status quo. They are change agents with a distinctly antiestablishment tinge. Elizabeth Cady Stanton, Eugene V. Debs, and Martin Luther King, Jr., are Miroff's examples of dissenting leaders. The heroic leader "assumes the mantle of democratic champion to pursue a self-aggrandizing role that jeopardizes democratic public life" (p. 7). The leader presumes to be the larger-than-life embodiment of the people; and the masses are spectators, not democratic citizens. Theodore Roosevelt and John Kennedy serve as examples of heroic leaders.

The case studies are uniformly strong and integrate the concerns for leadership style with the stories of each individual leader. Especially valuable are the chapters dealing with Lincoln and King. Miroff is at his best when assessing the contributions of these two men, whom he clearly admires.

Given the rich variety of leadership models in the United States, Miroff stresses that "if democracy is in need of nurturance by leaders committed to democratic values, then how Americans conceptualize leadership becomes all the more important" (p. 350). To accept the aristocratic or heroic models of leadership is to accept an elite model of leadership incompatible with democratic values. Miroff opts for some variation on the democratic and dissenting models. "We possess," he writes, "a rich and complex tradition, and it includes exemplars of democratic and dissenting leadership that can, with suitable adaptations, still be emulated" (p. 358). If we are truly concerned with creating a healthy democratic leadership, we must remember that "greater American leadership rests on fuller American citizenship" (p. 358).

In Miroff's vision, the only form of leadership compatible with democracy rests on respect for the citizen, the role of the leader as teacher and principled visionary, empowerment of the people, the promotion of egalitarianism, and the meshing together of the masculine and feminine sides of the individual (Lincoln's forté, according to Miroff).

While stressing the importance of strong citizenship, Miroff gives us few clues as to what, in the citizen, responds to different types of American leadership. What needs do these different types of leaders satisfy in the citizenry? Are there times when we might, quite legitimately, want—and even need—strong or heroic leadership? Under what circumstances might aristocratic leaders elevate the tastes and ways of the masses? Is democratic leadership a leadership for all seasons? A work of this scope and ambition cannot be without its faults, but those faults are minor compared to the wealth of insights contained within these pages.

Icons of Democracy is a most impressive work, which should serve as a major contribution to the study of American politics. Especially useful for advanced political science courses in leadership studies, the presidency, and American political thought, this book is an example of outstanding scholarship and should have a significant impact on the study of American political leadership for years to come.

Loyola Marymount University MICHAEL A. GENOVESE

Politics, Process, and American Trade Policy. By Sharyn O'Halloran. Ann Arbor: University of Michigan Press, 1994. 200p. $37.50.

The purpose of this book is to document, historically and with a few econometric models, the extent to which we can explain changes over time in U.S. trade policy by invoking the structures and procedures of the U.S. Congress, particularly its willingness to delegate its constitutional power over foreign trade to the executive.

The book begins with a succinct summary of some of the strands of trade policy literature, though the survey is not with out error. The pressure group model does not need to assume that capital is mobile (as the author asserts on p. 13). The mobility or otherwise of factors simply determines whether interest group demands are factor-specific or industry-specific, which, in turn, depends partly on whether one wishes to explain policy in the long or the short term.

Chapters 3–5 specify and test three hypotheses. The first is that when a new party attains power, the shift in trade policy will be larger than that predicted by a simple median voter model, because the median party member is some distance from the median voter (p. 29). This is tested in a time series regression of average tariff levels on party control of the Congress, with a control variable (GNP) to account for interest group influences. The author concludes that "even after accounting for changes in constituency demands, party significantly influences the tariff" (p. 68). One may question whether GNP is an adequate proxy for interest group effects, and the amount of variation in the tariff level that the author explains is very small for time-series data. I also believe the author might better have used tariff changes—rather than levels—as the dependent variable, since overall levels are the result of a very long historical accretion.

The next two hypotheses are that Congress will delegate more to the executive when it has similar preferences and that protection will decline with more delegation, since the president speaks to a larger constituency and may favor freer trade. The econometric test cited (chap. 5) shows how Congress delegates more trade authority to the executive when then economy is healthy and that such delegation yields lower tariff levels. Again, one may question whether fluctuations in GNP measure the consistency of executive and congressional trade preferences, particularly when the author has already used GNP to measure interest group influence.

Chapter 5 also provides a history of trade bills to 1984. The last chapter contains an admirably clear discussion of the evolution of "fast track" from its introduction in 1974 up to the vote on the North American Free Trade Agreement in 1993 and how Congress uses "fast track" provisions to reduce uncertainty and acquire information, while still retaining a great deal of control over trade policy delegated to the executive.

This book should be of interest to scholars of Congress and trade policy alike, and the argument that "institutions matter" is surely convincing. The author's attempt to separate congressional process and interest group influence on U.S. trade policy is less successful, since the latter is usually more amenable to cross-sectional test than to time-series tests.

University of Iowa JOHN CONYBEARE

Community Power in a Postreform City: Politics in New York City. By Robert F. Pecorella. Armonk: Sharpe, 1994. 240p. $45.00.

This monograph focuses on how the "inherent divisions between citywide and community-based interests" are "a central focus of regime transformation in New York" (p. 195). It offers both an analysis of historical trends in administrative and political centralization and decentralization and an assessment of the city's system of *community boards* as of the mid-1980s. By putting the city's current efforts at administrative decentralization in a broad historical and theoretical framework, Pecorella reminds us that the city continues to be a fascinating, if often frustrating, laboratory of urban democracy.

Pecorella distinguishes three periods in the New York's political development, each triggered by a fiscal crisis growing out of the breakdown of prevailing governing arrangements among business elites, political leaders, city agencies, and the attentive public. In Pecorella's reading, the era of machine politics began in the 1870s and lasted through the 1920s. Its rules of governance, or regime, centralized influence over administrative practices in the hands of party leaders, who struck deals with business elites, while decentralizing political ties to neighborhoods through ward and patronage politics.

The entropic tendency of ward politics, compounded by Tammany's inability to reproduce skilled leadership at the top, opened the way, during the Depression, for reform intervention. Led initially by Fiorello LaGuardia, reformers cut out the influence of particularistic interests and concentrated power in newly professionalized bureaucracies. Over time, however, this reform regime led to the fragmentation of administrative functions and hampered the ability of citywide political leaders to exercise central authority over policy. Following Martin Shefter's work, Pecorella sees the New York City fiscal crisis of the mid-1970s partly as a consequence of the inability to control demand for these functionally specialized services and partly as a strategic moment in which business elites, acting through fiscal monitors, reimposed control over the fragmented bureaucracy of the reform era.

At the same time, pressure from below had also welled up against autonomous bureaucracies. Demands for community control, citizen participation, and greater responsiveness to neighborhood concerns led those who reformulated New York City's charter in 1975 and 1989 to formalize a system of 59 community boards with advisory influence over city budgeting, land use decisions, and service delivery. Because it reforms the reform regime, Pecorella terms this period the "postreform" regime. He argues that it is characterized by the centralization of overall fiscal control in the office of the mayor and the fiscal monitors and the review of agency practices by geographically decentralized bodies.

After criticizing the antidemocratic tendencies of fiscal centralization, Pecorella turns his attention to how well this community board system is functioning. Drawing on original research on the success rate of community board capital budget initiatives and a survey of board members, he concludes that community boards function as vehicles for administrative cooptation, not community empowerment. Board budget proposals, for example, are far more likely to be adopted when a city agency cosponsors them, which middle-class boards are far more likely to achieve than boards in poor minority neighborhoods. Board powers over land use decisions are only advisory; while they have considerable influence, they have also been ignored or overruled at key moments.

The survey of board members finds that a substantial proportion favor strengthening the boards' influence over decision making, particularly on land use matters. Blacks held much stronger views on this matter than whites. While Pecorella concludes that this aspect of the postreform era has had only limited impact on agency autonomy, he cautions us not to discount the indirect influence that the requirement for community consultation has over the content of agency decisions.

In the main, Pecorella's historical interpretation follows that of other scholars who have studied the city's political development, especially Theodore Lowi, Wallace Sayre, and Martin Shefter. While not breaking new ground, he does bring together ideas put forward by these scholars at widely different times and circumstances. Pecorella's categories often seem overgeneralized (e.g., characterizing Mayor Wagner as the apex of reform and discounting the influence of regular Democratic political organizations in New York City between 1945 and 1965), but his approach has the merit of asking us to look for the large historical uniformities and tensions in urban political development.

Pecorella's greatest contribution is to remind us that underneath all the talk about racial polarization and fiscal austerity in New York City politics, the city has had an important experiment in administrative reform under way for three decades. The community board system sought to tame bureaucratic behemoths like the Department of Transportation or the Human Resources Administration and sensitize them to the almost infinite variation of neighborhood needs and wants. Pecorella is ambivalent about the results. It failed to meet the initial hopes and expectations that it would dramatically change the way agencies go about their business; but it has required them to consult neighborhood representatives, enabled the latter to put their concerns on the table, and encouraged agencies to find ways to accommodate these concerns. This "integrative decentralization," he concludes, "fulfills the promise of postreform politics" (p. 194).

City University of New York JOHN MOLLENKOPF

Hostages of Each Other: The Transformation of the Nuclear Power Industry after Three Mile Island. By Joseph V. Rees. Chicago: University of Chicago Press, 1994. 238p. $24.95.

Rees provides a fascinating account about how a traumatic event helped reshape the nuclear power industry. He describes the evolution of the institutional framework from one inherited from the management of fossil-fuel plants to a framework more suited to the dangers inherent in the operation of nuclear power plants. Rees contends that institutional adaptation has dramatically improved the industry's ability to avoid catastrophe, although Rees's optimism extends only to the adaptive process. Rees remains critical about the safety of nuclear plants, noting in particular the potential for operator error that still exists and the unfinished task of instilling professionalism in plant operators.

As in his previous book on the Occupational Safety and Health Administration, Rees focuses on conditions that support successful self-regulation. He analyzes the way in which incentives, beliefs, market structure, and organizational patterns define the safety problems that government regulatory policies attempt to resolve. The basic argument is that "communitarian regulation" based on control systems backed by moral imperatives are more effective than rule-oriented regulation, particularly in ensuring the safe operation of complex technologies. Self-regulatory institutions are more capable than government agencies to gain these advantages, although such institutions do not automatically succeed even in the most clear-cut case that Rees presents. After the Three Mile Island disaster, nuclear power companies were indeed "hostages of each other" in that another safety disaster at any plant would severely affect all the other plants.

Rees argues that successful self-regulation evolved primarily because of institutionalized peer pressure at the chief executive officer level, combined with the development, through the Institute of Nuclear Power Operations (INPO), of task definitions, authority structures, performance measures, and shared information links. Within a relatively short period, these developments changed the problem-ridden engineering culture and fragmented authority structure inherited from the fossil fuel past of the utility industry into a more professionalized culture capable of learning from all plants, with a normative commitment necessary to put the lessons to work. Rees presents the currently popular management philosophy about the need for a management culture but does so at a convincing level of detail. He describes the specific guidelines and evaluation criteria that were developed and carefully analyzes how they were successfully worked into the motivational framework of plant operators. Rees refers to this process as "building a structure of responsibility," one of this book's themes that may be particularly relevant for managers.

Rees's study is not important because the case captures a "typical" regulatory situation. The threat of survival to entire industry after Three Mile Island that helped resolve the "collective" problem of safety has few parallels in other market situations. Nor can we be certain that the degree of success achieved under INPO was related to the factors Rees documented as part of the evolution or whether, given the kind of threat facing the industry, many other possible developments would also have been consistent with these developments. This is a generic problem of looking at "success stories" for evidence about what works.

The significant contribution of Rees's study lies more in the rich example it provides of the institutional analysis of regulation. The brief outline of the theory in the concluding chapter does not do justice to the rich, implicit theory that has guided the narrative. The theory certainly deserves a more formal presentation. Rees's case study complements theoretical attempts in the field of political economy (e.g., Douglass North's institutional studies and Gary Miller's *Managerial Dilemmas* [1992]) to specify, with some precision, the role of norms, legitimacy, leadership, and "moral community"—the stock and trade of political philosophy—in the governance of firms and the marketplace.

State University of New York,
Stony Brook
JOHN T. SCHOLZ

Reclaiming the Federal Courts. By Larry W. Yackle. Cambridge: Harvard University Press, 1994. 297p. $39.95.

The Supreme Court's substantive decisions continue to shrink the meaning of a number of the constitutional and statutory rights that the Warren Court expanded several decades ago. In this new book, Larry Yackle passionately argues that the Supreme Court's procedural decisions have eroded protection for federal rights just as significantly, by restricting access to the federal courts. The Rehnquist Court, following the legal process school of jurisprudence, has limited standing to sue, federal subject-matter jurisdiction, and habeas corpus relief, among other things, preferring to exclude rights claims from court entirely or to send them to the state courts. The problem, in Yackle's view, is that state courts are institutionally incapable of protecting federal rights with sufficient vigor. The consequence is that "the fundamental framework of American government is now very much at risk" (p. 2).

Yackle's tone is urgent and passionate. In his revealing epilogue, he notes that as a student, he worked on federal habeas corpus petitions for prisoners at the Leavenworth Penitentiary and, he confesses, "the lesson I learned at Leavenworth is that the federal courts are special" (p. 213). The current Supreme Court insists, however, that the federal courts are not special and that state courts perform just as well at enforcing federal rights. But Yackle argues that state courts are either hostile to claims based on federal rights or are ill equipped institutionally to handle them. He proposes that Congress pass new legislation to reopen the doors of the federal courts, and he suggests specific language to accomplish that task.

The basic assumption of the book—that access to the courts and the institutional capacity of courts are as essential for the protection of rights as are sympathetic judges—is very important but somewhat underdeveloped. Yackle claims, basically, that the federal courts (no matter what the political preferences of their judges) are more friendly to federal rights and more capable of dealing with them than are state courts (no matter what the preferences of their judges) (chap. 1). Why? Because of the superior quality of federal judges and support staff and because federal courts have greater resources and are more insulated from popular pressures than are state courts. Yackle is unconvinced that the recently growing liberalism of some state supreme courts will make state courts useful to litigants claiming federal rights. This is so, he says, because only a few state supreme courts have become more active, because state supreme courts have access to such issues only on appeal (so that poor litigants must finance their claims through lower state courts first) and because state court liberalism on questions of *state* law may not mean that they will rule liberally on federal law. He argues, for example, that state supreme court activism on school financing reflects the existence of legislative log-jams more than it reflects judicial concern for rights (p. 219, n. 3). Perhaps. But the same could be said of past federal court activism on some issues, as well. Oddly, Yackle consigns this crucial argument over the comparative capacity of state and federal courts to a few introductory pages and a series of notes at the beginning of the first chapter.

Yackle devotes most of the book to a complex analysis of the Supreme Court's decisions in four procedural areas that the Court uses to exclude claims from federal courts: justiciability, especially the doctrine of standing (chap. 2); federal subject-matter jurisdiction (chap. 3); abstention (chap. 4); and the writ of habeas corpus (chap. 5). On these legal matters, Yackle's argument is clear and persuasive: the Supreme Court's procedural decisions have significantly restricted access to federal district courts. He proposes specific statutory language to end each of the Court's restrictive procedural rules. His proposals would decisively reverse current Supreme Court interpretations of justiciability and federal court jurisdiction. For example, he would grant standing to any citizen who claims an ideological interest in "seeing that conduct conforms to federal law" (p. 83). Such a rule would expand greatly the opportunity for public interest litigation.

Yackle's proposals are refreshingly bold, but perhaps almost naive. He assumes that "the time is coming when we Americans will have done with the ideological conservatism that slipped into power when the mainstream liberal consensus collapsed in the mid-1970s" (p. 3); and when that happens, Congress will happily reopen the federal courts to federal rights claims. The tone implies that such a future is just around the bend and that therefore the time is ripe for the entertainment of proposals to accomplish the task. Clearly, Yackle could not have predicted the Republican takeover of Congress; but in the present context of renewed interest in governmental and judicial federalism, his proposals sound strangely out of place. His critical analysis, however, is not. While Yackle's proposed solutions may fall on deaf ears, his thorough criticism of judicial policies closing the doors of the federal courts to the accused and to some public interest litigants is an important contribution to legal scholarship on the Supreme Court.

Indiana University
CHARLES R. EPP

COMPARATIVE POLITICS

Public Politics in an Authoritarian State: Making Foreign Policy During the Brezhnev Years. By Richard D. Anderson, Jr. Ithaca: Cornell University Press, 1993. 266p. $39.95.

With this well-argued and provocative volume, Richard Anderson has made a significant contribution to our understanding of the foreign policymaking process in the Brezhnev-era Soviet Union. Drawing from an unexpected source for a study of Soviet politics, Anderson borrows from the theory of political competition in electoral polities to explain the development of Soviet foreign policy in the late 1960s and early 1970s. In so doing, Anderson aims to explain not only why the Soviet Union adopted the ultimately counterproductive policy of *offensive détente* but also to demonstrate that political competition among Soviet Politburo members followed dynamics similar to those found in competition between politicians in electoral polities and that this competition crucially shaped Soviet foreign policy.

Anderson's primary task is to explain the adoption of "the foreign policy of offensive détente" by the Brezhnev Politburo. Offensive détente, as originally defined by Jack Snyder, refers to "the Soviet effort, after 1969, to couple U.S.-Soviet détente in bilateral and European issues with interventionism in Third World conflicts, an aggressive arms build-up, and exclusion of Western influence from Eastern Europe" (p. 13). This policy proved counterproductive, says Anderson, because it provoked strong balancing reactions from other powers, led to an increasingly large military budget in a time of slow economic growth, and forced the USSR to subsidize its East European allies heavily. This policy, then, actually weakened the international position of the USSR. Other authors have viewed this policy as a responsive adaption to international conditions, says Anderson; but they slight evidence that the policy could more accurately be seen as a tactical compromise among Soviet Politburo members competing for authority and control over policymaking.

Anderson's main line of argument revolves around the assertion that like politicians in electoral polities, Soviet Politburo members had to *go public* to gain support. Building on George Breslauer's conception of "authority building" by Soviet leaders, Anderson contends that Soviet leaders could not maintain their positions simply by means of their control over appointments, as the classic "circular-flow-of-power" argument held. Rather, to govern effectively, they had to build support within the state bureaucracy, principally by "winning reputations among officials for competence in devising solutions to policy problems and for artfulness in brokering the conflicting demands of functionally specialized bureaucracies" (p. 11). Going public with a vision of the appropriate path for the development of Soviet socialism, says Anderson, was an effective means for Soviet Politburo members to build such support among the bureaucrats.

When politicians go public in a competition for support among the members of a selectorate who have limited time and attention, they must rely on symbolic appeals. In particular, they must rely upon synecdoche—individual examples that stand for more general propositions, even if the particular example is not typical. In their competition for support among bureaucrats, then, Soviet Politburo members publicly advocated symbolic visions of the development of the USSR and its role in the world. Each Politburo member thus shaped his recommendations for Soviet grand strategy not only on the basis of objective international conditions but also in view of how consistent such recommendations would be with the symbolic vision of the Soviet future he was promoting. From this perspective, Soviet grand strategy in the Brezhnev era was not so much the result of adaptation to objective international circumstances as it was the outcome of bargaining among Soviet leaders using symbolic visions to compete for power and influence.

Anderson then explains the Soviet policy of offensive détente as the result of bargaining among the four principal contenders for political power following the ouster of Khrushchev in 1964: Leonid Brezhnev, Aleksei Kosygin, Mikhail Suslov, and Nikolai Podgorny. The middle section of the book is devoted to an articulation of each leader's contrasting vision of socialism and a matching grand strategy. The book then goes on to trace the development of Soviet foreign policy in the late 1960s and early 1970s, demonstrating a clear correspondence between observed change in Soviet foreign policy on issues contested by the four senior Politburo members and the expected outcome of bargaining among their public stances (p. 236).

Anderson uses this evidence to come to two linked conclusions. First, going public constituted an important political activity for Soviet Politburo members, which crucially affected their position and influence over policy. This assertion in itself will prove controversial with many readers, one suspects, who will view this use of the theory of political competition as a misapplication of Western social science theory to the alien terrain of Soviet politics. But such readers will have to contend with Anderson's well-documented evidence that Soviet leaders did go public; and his findings clearly undermine traditional interpretations of the functioning of the Soviet political system, which emphasize its institutional distinctiveness. Although Anderson's circumstantial case for the importance of going public is strong, one would like to see additional evidence (as might be found in the slowly opening archives of the Communist party of the Soviet Union) that could more clearly demonstrate the impact of public advocacy and bargaining among the top members of the Brezhnev Politburo. Future research on this era might fruitfully attempt to validate Anderson's conclusions further. Lacking current access to such evidence, however, Anderson has made a very strong case for his view.

Anderson also uses the case of the Brezhnev-era Politburo to argue that public competition among politicians is not limited to systems with electoral institutions but is a broader phenomenon. The process of recruiting political allegiances, he argues, is shaped by, but not caused by, the institutions of a political system. It is something of a universal constant in politics (p. 244). Although his circumstantial case for the influence of political competition on Soviet foreign policy in the Brezhnev era is quite strong, Anderson may be stretching a bit far in reaching such general conclusions. He looks at only one nonelectoral political system and only over the span of a few years. Even the Soviet Union itself in other historical periods may not exhibit the characteristics that he observes in the Brezhnev era. His suggestion, though, should certainly not be dismissed and offers an interesting avenue for future research.

Second, Anderson concludes that offensive détente emerged as the result of "tactical" compromises among the publicly articulated (and highly symbolic) positions of the senior members of the Brezhnev Politburo. Since these public positions were designed for appeal to the mass of Soviet bureaucrats, consistency with the leader's proposed domestic vision, and ease of transmission, they were not accurate representations of the international environment.

The policy that emerged from the bargaining process, then, was not likely to be adaptive to international conditions. Symbolism displaced objective problems, and the separation of policy issues from one another, while facilitating bargaining among the members of the Politburo, led to a failure to anticipate substantive interactions among different elements of foreign policy. The result, offensive détente, combined internationally incompatible policies (e.g., East-West détente and support for revolutionary change in the Third World) in order to achieve domestically acceptable bargains among the competing public positions of the four senior Politburo members. Given the substantial recent literature on the influence of domestic political structure on Soviet foreign policy, this conclusion will be far less controversial than its counterpart. It does represent an interesting departure, however, since earlier accounts did not explicitly address the effects of bargaining among politicians who were competing publicly, using symbolic images to build political support.

Despite the insights that this approach brings, the reader is left at the end to wonder whether the distribution of political forces in the Soviet Union had any impact on the grand strategy that emerged from the bargaining process that Anderson carefully traces. If politicians had to differentiate themselves, surely the positions they took were related to their impression of the opinions and interests of the selectorate to whom they were appealing. Members of representative assemblies, after all, tailor their electoral appeals to the district in which they are running. Soviet Politburo members, if they acted similarly to electoral politicians, should also have shaped their appeals to fit the interests of their "constituents," the bureaucrats. Certain public positions—those which defended the interests of the entrenched Stalinist bureaucracies of heavy industry, the military, and the Party apparatus—should, intuitively, have found more support, simply because they had more bureaucratic constituents. Earlier research has clearly demonstrated that these sectors of the state *apparat* dominated the bureaucracy as a whole, from the level of the Central Committee on down.

From this perspective, the public bargaining process that Anderson meticulously traces is only an intervening variable that filters, albeit imperfectly, the underlying policy preferences of the bureaucrats into state policy. While policy may have been a bargain that resulted from politicians' public search for constituent support, then, these public positions may have depended as much on the underlying structure of constituent interests as they did on the dynamic of competition itself. Anderson himself suggests that competitive politics may be an intervening variable, rather than an independent causal factor (p. 252). Offensive détente, then, may have been the result of various interest groups lobbying the Politburo members, who responded by shaping their public positions accordingly. The search for constituent support, then, is only one (albeit an important) link in the chain. These shortcomings, however, do not detract from the important overall contribution that Anderson has made to our understanding of the foreign policy-making process in the late Soviet Union.

University of Texas, Austin SCOTT PARRISH

Governing Together: The Extent and Limits of Joint Decision-Making in Western European Cabinets. Edited by Jean Blondel and Ferdinand Muller-Rommel. London: Macmillan, 1993. 338p. $59.95.

This is the latest in a lengthening list of volumes produced by the research group on cabinet government centered on Jean Blondel and featuring a familiar roster of authors and coeditors. As the subtitle suggests, it is concerned with how cabinets decide, measuring the theoretical ideal type of collective cabinet decision making against the constraints imposed by prime ministerial power and departmentalism.

This particular research group has worked together for a long time now, and the result is another volume with a coherence of style and approach that other collaborative projects would do well to emulate. Although most of the authors are country specialists, they cut the cake thematically, producing chapters on, among other themes, the difference between decision making in single party cabinets and coalitions (Frognier); the organizational structure of cabinets (Thiebault); the flow of cabinet business (Burch); links between parties and ministers (de Winter); individual ministerial styles (Blondel); the role of finance ministers (Larsson), prime ministers (Muller, Philipp, and Gerlich) and prime ministerial staff (Muller-Rommel); and conflict resolution (Nousiainen).

The material for each chapter was based both on secondary sources and on a series of structured face-to-face interviews with 410 cabinet ministers in 12 countries. This is stated to be "about half" of the universe of able-bodied ex-ministers in the countries in question (p. 309): no details of sampling or selection procedures are provided, and there is no discussion of potential selection bias in the "sample." Data analysis is typically confined to marginal totals and simple tables, and no statistics are provided for these. It is sensible, therefore, to take the data as suggesting possibilities, rather than as the basis for reproducible generalizations.

We thus have an experienced and able group of authors, working in a well-coordinated manner on an interesting and important theme and using original, if rather underspecified, primary data. So far, so good; but unfortunately, the book lacks one essential ingredient—a systematic theoretical context. To be sure, there is a general idea behind the study, namely that life in single-party cabinets is different in many ways from life in coalitions. But this idea is not in itself a theoretical framework for the project. The absence of such a framework is an unfortunate flaw in an enterprise that, commendably, sets out to be much more than a collection of thickly descriptive case studies. A considerable part of the discussion is couched in terms of generalizations and taxonomies, rather than detailed information, yet the justification for these abstractions is typically a rather all-embracing appeal to secondary literature, common sense, and/or received wisdoms.

Consider, for example, the ministerial questionnaire used to generate most of the data on which the book is based. In the absence of an explicit theoretical context, it is difficult for outsiders to know what to make of much of this and therefore what to look for in the data generated. To take just a single instance of this problem, we find that 60% of the ministers interviewed were satisfied with "the general working of the cabinet" (p. 187). But why were ministers asked this question? Is a 60% satisfaction rate high or low? What do we learn from it?

This lack of theoretical focus has the effect that while much of the institutional detail in each chapter is of very great value to those who are interested in cabinet government, discussions of the ministerial survey sometimes lack a clear direction and fail to take full advantage of what ought to have been a unique source of data on executive politics in Western Europe. The impression is sometimes created that a given author has been allocated a particular part of the data to discuss and discusses this because it is there, rather than in an attempt to get to the bottom of a set of burning research questions. In this sense, the reader sometimes gets a feeling of wasted opportunity: interviewing 410 ex-cabinet ministers is a huge job to undertake without a very clear sense of purpose other than a general commitment to throw light on a hitherto underresearched area.

Despite this lack of theoretical focus, however, the authors have done us a very valuable service in paying at least a first visit to a range of important aspects of the problem of how cabinets decide. The empirical analysis is based on a large and unique set of interviews with cabinet ministers. The authors know their fields. The desire to take this problem into the realm of comparative analysis is admirable, and everyone with an interest in cabinet government will get something out of this book.

University of Dublin MICHAEL LAVER

The Vogue of Revolution in Poor Countries. By Forrest D. Colburn. Princeton: Princeton University Press, 1994. 105p. $19.95.

Most works on revolution, whether allegiant to Marxist or modernization theories of development, have, to date, adhered to explanations of revolution's causes that stress such structural factors as peasant or worker discontent, the gap between expectations and achievements, the distinction between the building of institutions and the demands placed upon them, and the impact of international pressures on unwieldy agrarian bureaucracies. More recently, others have emphasized the self-interested calculations of actors such as peasants as generators of revolutions. Forrest Colburn's *Vogue of Revolution in Poor Countries* focuses, instead, on the salient role of ideas and of political agency in the genesis of revolutions, on the ensuing construction of new states and societies, and even, given the frequent discordance between revolutionary ideals and reality, on the ultimate failure of virtually all revolutions.

Colburn is particularly struck by the "remarkable parallels among revolutions in countries otherwise remarkably different from one another" (p. 5). This he explains by the vogue of socialist ideas that took hold among the small intellectual elites of poor countries in the years after World War II, ideas that, ironically, derived mainly from the very countries of the West whose economic and political domination these same elites were seeking to overturn.

The universe of revolutions encompassed by the author includes all those, and only those, occurring in so-called poor countries since 1945—22 in all. The revolutions are not treated individually but, rather, used as illustrative examples for the author's successive discussions of such aspects or phases of revolution as their intellectual culture, the seizure of power, the building of the state, its manifold problems, and its ultimate collapse. The principal contribution of the book—and it is a significant one—is to bring forcefully to the attention of scholars of revolution the commonality of revolutionary ideas across a wide diversity of regions and cultures and, above all, to supplement structural and rational choice approaches to revolutions by highlighting the role of ideas in their generation (and ultimate demise). This is not an argument that ideas are everything but rather that their importance has been neglected in the literature. The fact that so many revolutions are embraced by the work is likewise a mark of distinction, especially given the fact that most studies of revolution cover only one or a few of the major revolutions or draw their examples exclusively from them.

At the same time, although the author notes that he will attempt to draw more or less evenly from all of these revolutions, this is not really the case. Ethiopia and Nicaragua (in both of which the author resided), as well as a few others, get the bulk of the illustrative attention, while others, like those of Burkina Fasso (1983), North Korea (1948), and Benin (1972) get little mention. One might also quibble about the author's inclusion of some revolutions (e.g., Egypt's "revolution from above") when other, seemingly comparable ones (e.g., 1968 Peru or 1969 Libya) are omitted.

More important, there is no systematic comparative analysis of the various revolutions (as there hardly could be in a book of 105 pages) nor any effort to delineate subtypes. Thus, China is lumped together with Burma, Grenada, South Yemen, and other seemingly very dissimilar phenomena. On one level, this is useful to the author's purpose, arguing the similarity of ideas among so many countries and revolutions; yet at another level, it may strain credulity for some readers. While Colburn does make an effort to note exceptions, particularly with respect to Iran, there is no separate treatment of the element of diversity in postwar revolutions or, for that matter, of why in some cases (e.g., China) ideas would seem to play a driving role in revolution, while in others (e.g., some of the military regimes), they seem more a post facto rationalization of a drive for power.

Finally, there is no real discussion, other than an acknowledgment, of the relationship between ideas and other variables. This, in turn, means that we get no discussion of such a central consideration as why other poor countries, in a similar time frame and presumably affected as well by "the vogue of revolution," have failed to produce revolutions.

In sum, the germ of an argument is certainly here and is generally aptly put. As such, it is a highly pertinent challenge to the mainstream scholarship on revolution. However, it remains for the future for Colburn or others to expand on the thesis in a fully comparative manner, taking into account the manifold variations and dimensions of modern revolution.

Rice University ROBERT H. DIX

The Decline of Communism in China: Legitimacy Crisis, 1977–1989. By X. L. Ding. New York: Cambridge University Press, 1994. 230p. $49.95.

The principal question that interests X. L. Ding—a People's Republic of China citizen until the mid-1980s and more recently a Research Associate at Harvard's Fairbank Center for East Asian Research—is why, despite significant economic reform and rising prosperity from the late–1970s, the People's Republic experienced its greatest period of popular protest in 1989. The basic answer, Ding suggests, is that the essential contradictoriness of the top leadership's 'dual-traffic' policy (i.e. economic liberalization, political conservatism) led to a legitimacy crisis in mainland China in the late 1980s.

A major aspect of this growing legitimacy crisis that particularly interests Ding is the role played by what he calls the intellectual counterelite. This he subdivides into four groups: the radical marginal intellectuals, who were mostly born around 1949, acted as Red Guards during the Great Proletarian Cultural Revolution, and did not typically have a tertiary-level education; the critical cultural intellectuals, mostly research fellows in research institutes, university staff and students in the humanities and social sciences, and editorial staff of serious journals and publishing houses; the politically active technical intellectuals, mainly from the natural sciences and engineering; and the independent-minded official intellectuals, primarily party members and officials, and senior members of the cultural, educational and media establishments. Ding argues very persuasively that as the Chinese leadership constantly sought new bases of legitimacy—whether in rationalism, socialist morality, patriotism or stages of socialism—so the intellectual counterelite would produce a counter argument and campaign.

Another major plank of Ding's argument is that much of the counterelite's undermining of official campaigns was possible precisely because the senior leadership permitted some liberalization in the political sphere. Given the constraints, much of the counterelite's attack was made within the official institutional framework, and could only proceed as far as the leadership would tolerate. Given this, Ding argues against the overly stark contrast implied by the 'state vs. civil society' image adopted by so many analysts of late communism, preferring instead the concept of 'institutional parasitism'.

One other important component of Ding's argument is that the top elite typically attempted to prove how well communist China was performing by using flattering vertical comparison (i.e., comparing contemporary China with the past), whereas the counterelite mostly drew disparaging horizontal comparisons, whether with East Asia (especially Taiwan), or with the USSR and Eastern Europe in the era of *glasnost* and *perestroika*.

Ding's book makes a valuable contribution to the literatures on legitimacy and legitimation crises (both generally, and specifically in China), civil society under communism, and revolution, though in each case the author sometimes oversimplifies the arguments of others in order to strengthen his own position and emphasise its originality. For instance, whilst his criticisms of the civil society debate in communist and post-communist studies are largely valid, he is not the first to have argued that there was a symbiotic relationship between the senior authorities and within-system oppositionists, or between the authorities and the masses (this case is made in at least one of the chapters in R. F. Miller's 1992 edited collection, *The Developments of Civil Society in Communist Systems*, a book Ding cites). Moreover, in portraying most of the existing literature on legitimacy and legitimation as drawing a basically two-fold distinction between rulers and ruled, Ding is creating a false impression; *most* of the literature draws a three-fold distinction between chiefs, staffs and the general public.

Another weakness is that Ding sometimes blurs conceptual distinctions that are critical if we are better to understand legitimacy and legitimation crises. For example, there is some confusion between regime and system on p. 197–8; a sharper delineation would add another dimension to our understanding of the differences between the Democracy Movement of the late 1970s and the events of 1989. Whilst Ding is clearly familiar with Habermas's work, he does not explicitly draw the important and enlightening distinction Habermas does between rationality and legitimation crises. Finally, some explicit consideration of the differences between legitimacy and legitimation (and of crises of both) would have been welcome; Ding treats the concepts as if they are synonymous (see esp. p. 13–17), which detracts from what is mostly a sophisticated and nuanced argument.

It is unfortunate that Ding opts to describe the form of opposition engaged in by so many intellectuals as parasitism, since this term generally has a pejorative connotation. The form of opposition described was in many ways necessitated by the nature of the communist system, and it could well be that it is in many cases more successful over time at delegitimizing a communist system than is either overt dissidence or mass protest on the streets. Moreover, Ding's clear and convincing argument about symbiosis is somewhat muddied by his references to parasitism.

The most frustrating aspect of this book is that the analysis essentially concludes with the 1989 clamp down. Whilst he does provide a justification for this (p.4), most readers will wish that Ding had been more adventurous, and had speculated about the degree of legitimacy both the regime and the system have enjoyed in the 1990s. Have the problems of early postcommunism in the former Soviet Union and Eastern Europe enhanced the legitimacy of communism within China? Or is there a legitimacy crisis anyway at present in the People's Republic of China? If there is, what are the reasons for this, and can the regime and system survive? Given that the initial stages of the public unrest of 1989 were associated with Hu Yaobang's death, it is unfortunate Ding does not consider the possible effects on the communist system of Deng's death. If legitimacy and legitimation crisis theory is to be taken more seriously, as Ding justifiably argues it should be, it must be applied to future possible scenarios.

The above flaws notwithstanding, Ding has written an impressive book. It is both well-written and well-structured. It is also a work of true scholarship, and constitutes a largely successful blend of deep theorizing and detailed empiricism. His appendix on methodology is refreshingly honest and sensitive.

University of Melbourne LESLIE HOLMES

Forging Industrial Policy: The United States, Britain, and France in the Railway Age. By Frank Dobbin. New York: Cambridge University Press, 1994. 262p. $39.95.

Politics and Industrialization: Early Railroads in the United States and Prussia. By Colleen A. Dunlavy. Princeton: Princeton University Press, 1994. 303p. $39.50.

These two studies, one by Frank Dobbin, a sociologist, and the other by Colleen A. Dunlavy, an historian, engage a question that has been an object of great interest and controversy in political science over the past several decades—the relationship of political structure to industrial development and policy. Perhaps the most dominant theme in this literature is the role of interests. Interestingly enough, both authors, coming from different directions, basically reject this view. They both seek to make a case for looking beyond the immediate context of politics and the politics of interest to explain the variations in industrial policy across societies.

For Dobbin, the problem is viewed through the lens of his unhappiness with a literature that relies heavily on culture and tradition as residual variables to explain the variance left unexplained "after theoretically important factors are used to explain a tiny proportion of cross-national variance" (p. 1). He thus begins by rejecting the view of social actors that stresses that action is "governed by universal laws of interest and rationality" (ibid.). Dunlavy, on the other hand, comes to the issue with a specific problem in mind—the paradox presented by the experience of the United States and Prussia with regard to railroad development in the first half of the nineteenth century. She notes that while the Prussian central state "had adopted a largely hands-off policy," the American state, in contrast, "had taken a comparatively interventionist stance" (p. 4). The more liberal state was the more interventionist. Why so, when we would expect it to be the other way around? She answers by saying that the key to the paradox "lies neither in the realm of national cultural nor in the actions of the state, but in the structuring presence of the two states themselves. . . . To contemporary nations, early American and Prussian railroad development shows how the constitutional structure—the larger, overarching structure of national political institutions—plays a constitutive role in industrial change" (ibid.).

This being said, the authors now take quite different paths. Dobbin's argument is that industrial policy is isomorphic to state institutional structure: "Policy approaches are reproduced because state institutions provide principles of causality that policy makers apply to new problems, and not simply because institutions give policy makers the organizational resources to repeat history" (p. 3). The isomorphism is generated by political culture that determines the "kinds of economic and industrial *problems* nations would perceive and by delimiting the *solutions* that nations would conceive to those problems" (p. 20). The values enshrined in the institutions that brought political order were brought to bear when it came to problems of economic order (p. 21).

To substantiate this view, Dobbin examines the development of state policy with regard to railroad development in the United States, Great Britain, and France in the nineteenth century. His premise is that each of these countries proceeded on the road to the development of an industry with relatively small technological variations in a manner that mirrored its political institutional values and structure. In the United States, political institutions reinforced the values of community sovereignty, while in Great Britain they reinforced individual sovereignty. French political institutions reinforced state sovereignty. Dobbin describes the isomorphic character of the structures of political values and railroad development policy. Each society's political culture leads its citizens to perceive a unique series of problems that are resolved in that society's political cultural image.

Dunlavy proceeds in a different manner. She tries to solve her paradox by comparing the role of the state in early railroad development in Prussia and the United States. After a careful examination of what she views as functional equivalents—the Prussian central state and American state governments—she concludes that the United States was considerably more interventionist in the 1830s and 1840s. Dunlavy compares different possible explanations. She concludes that it was not a question of difference of interests: "On the whole, neither the prevailing pattern of support for early railroad development nor the varied sources of opposition differed much in the United States and Prussia during these years" (p. 125). Nor were there significant differences in ideology or tradition concerning railroad or transportation development between the two countries. Instead, she argues that the difference between the two countries' policies was a function of their overarching political structures. The more liberal American political structure offered greater capacity for the expression of opposing interests: "This is why the American state promoted early American railroads with greater energy, and, because it promoted, it was able to regulate" (p. 131). The Prussian state could not promote railroads in the same way without yielding to demands for political liberalization. The cost of development would have made it necessary to call for cooperation of interests in raising new taxes. This would have opened the door for greater political participation. Prussian state officials thus found it prudent to allow private capital to organize railroad development with a modicum of state interference.

Dunlavy goes on to show how the unity of the Prussian state made possible the early formation of national railroad associations, while the decentralized character of the American system made the formation of a more or less permanent railroad association come quite late. These same holds true, she argues, for the diversity in technological styles between the two countries. The centralized Prussian state shaped a more homogeneous engineering community than the decentralized disparate engineering community produced by American diversity. Institutional structure, rather than political culture, is the dominant constraint in railroad development policy.

Both explanations have problems. Regardless of how it is defined, Dobbin's resort to political culture suggests that the agents of social action are conceived of as mere transition belts of internalized norms. Individuals very early on acquire a set of norms that governs their behavior regardless of setting. Everyone acts in a subjective atomized fashion, since their understandings are mediated by internal constraints unrelated to the immediate social context in which they exist in a practical way. Thus, the engine of isomorphism is unchanging norms that makes it possible for Dobbin to speak of Americans, French citizens, and British subjects as universals and thus reduce conflict to trivial disputes over the range of internalized norms. It also makes it possible to ignore historical events of the greatest magnitude.

This view amounts to a determinism that cannot be sustained by the tautology of the argument. The tautology lies in the fact that the existence of the norms he cites is deduced from the isomorphism itself. There is no independent evidence of internalized political norms being the most salient constraint on the perception and solution to problems. If Dobbin means to say that there is a common understanding underlying differences of interest, then he ought to make this explicit and show how this common understanding generates different interpretations.

What is there instead? Dunlavy has an answer: institutions matter. They are not merely the unmediated consequence of internalized norms and values but are independent variables in themselves. While individuals may have norms about what constitutes an appropriate problem and the knowledge required to solve it, institutional rules and sanctions dissuade them from pursuing those norms regardless of what others do. Dunlavy argues that the nature of the political structure provides significant boundaries within which agents may operate. It is true that at bottom, Dunlavy is arguing that social actors are impelled by their interests. But these interests are shaped by an interaction with a social context with greater or fewer options for action. Nevertheless, Dunlavy also leaves us in a quandary. Her argument is that a more liberal constitution produces the capacity for greater interests. But it is not clear what is precisely meant by *liberal.* Does the existence of variegated interests indicate liberalism? At any rate, Britain's relatively liberal constitution did not seem to have produced anywhere near as broad a range of interests as in the United States. Nor does this appear to be the case in France under the Third Republic. Is it possible that it is decentralization of state authority that, as Dobbin would have it, allows for the emergence of such a broad range of interests? Smaller units make possible the organization of interests by smaller groups of people needing fewer resources than are required at a national level.

Despite some shortcomings, both analyses make important contributions to our understanding of the nature of state industrial policy formation. Dobbin's analysis makes us aware of a truism often lost to us—that every society does not have the same conception of efficiency or rationality. These are social or political constructs, not universal inherent qualities of technology. Dunlavy, on the other hand, makes us aware of the role of political institutions as holistic structures in determining the specific boundaries within which action can be formulated and organized. The result is that these two volumes give us a refreshingly different and significant perspective on the problem of the state and its relation to industrial policy.

University of Chicago BERNARD S. SILBERMAN

Governing Rural England: Tradition and Transformation in Local Government, 1780–1840. By David Eastwood. New York: Oxford University Press, 1994. 311p. $55.00.

In the 1980s, the Thatcher government made it a central goal to control local governments. The government did so both by controlling the levels of taxation and spending the local governments could set and, in some instances, by abolishing metropolitan local governments controlled by the Labour party. Opponents criticized these choices in part by appealing to a long tradition of local governance in Britain. However, some centralization had to have occurred already, else the Thatcher government could not have exercised that control over local governments. Indeed, in more limited ways, central governments have tried to impose controls on local governments in Britain throughout the twentieth century. They have done so through the presumption that the localities are subject to statutes from Parliament.

David Eastwood argues that the initial subjection of the local governments to central control came from a combination of social crisis and changing ideology in the early nineteenth century. He examines changes in central-local relations through focusing on policy areas. He chooses two policy areas central to governance: the administration of the poor laws and the administration of criminal justice. Local administration of the poor laws became unworkable in the face of massive and widespread unemployment and falling wages. Local magistrates and parishes struggled to maintain the poor while also addressing the rise of Malthusian ideology that argued it was pointless to try to support the poor. Furthermore, an allowance system made poor relief sometimes simply an addition to extremely low wages, ending up as a subsidy to employers. Parliament took over the administration of the poor laws by enacting reform statutes, in particular, the Poor Law of 1834. In doing so, Eastwood argues, Parliament participated in a transformation of the meaning of what made governing elites legitimate. Local elites had governed based on an assumption that they were the best people because of their social status. With the intractibility of the local problems, Eastwood argues that the intervention of Parliament contributed to a meaning of governance as justified when based in effective management techniques. While he describes neither anything like today's concerns with expertise nor the jargon of quality management, Eastwood does suggest a move toward more central control and concern with effective management. Eastwood particularly develops the relationship between local governance and professionalization through Parliament in his discussion of police administration.

Police and prosecutions were organized differently from county to county. They were sometimes volunteers and often paid only through voluntary contributions. Local efforts to create professional police forces often ran into opponents who believed professionalization would encroach on liberty and would be pointlessly expensive. The late eighteenth and early nineteenth centuries saw an increase in criminal committals, possibly in part as a result of the growth of prosecutors' associations. They were private associations formed to prosecute crimes. They increased in number in the early nineteenth century as a result of a sense that crime was on the rise. Increasing criminal committals and concern about crime taxed local police. In addition, stories of police incompetence led some to question the existing organization. In 1839, Parliament enacted the Royal Constabulary Act to facilitate the creation of salaried police forces. On the one hand, this was a centralizing effort; on the other hand, Parliament tried to preempt the concerns of those who wanted to maintain the decentralized system of administration through magistrates. First, the counties could choose to adopt it or not. Second, the act would create a police force under the supervision of a chief constable. However, the chief constable would remain under the control of the local magistrates. In this way, then, Parliament worked through and with the existing forms of governance, the magistrates. However, that system also bears a resemblance to the modern strategy of relying on local governments largely as administrators of central government policy.

This book is a historical study based, in part, on close analysis of primary sources. The questions of institutional transformation that it addresses are crucial in political science. Eastwood emphasizes the role of social crisis in creating a need for change. But he also attends closely to the institutional factors that profoundly shaped changes. As recent developments in political science have urged, a historical framework for political science is necessary if we are to understand institutional change, rather than analyze static systems. In turn, institutional change is best understood through particular policy areas. This careful history provides just that type of analysis.

University of Denver SUSAN STERETT

Healing the Masses, Cuban Health Politics at Home and Abroad. Julie M. Feinsilver, with a Foreword by David E. Apter. Berkeley: University of California Press, 1993. 307p. $45.00 cloth, $17.00 paper.

"Everything Within the Revolution," Cuban Strategies for Social Development since 1960. Thomas C. Dalton. Boulder: Westview Press, 1993. 178p. $46.50.

Maintaining revolutionary loyalty through the provision of health and medical services to its population is one of the main features of Cuba's social development model since 1959. Consistent with its belief that socialism meant more than abolishing a market economy and eliminating private medicine, Castro's regime developed an impressive system of health and medical services through which basic human needs in health care were largely met. Feinsilver accepts the regime's view that "health care is seen as a basic human right and the responsibility of the state," with leaders defining improving (or declining) indicators "as measures of government efficacy" (p. 1).

A second aim of the health system was to enhance Cuba's international prestige, especially in the developing world, and to show the superior capabilities of revolutionary socialism in resolving problems typical of developing societies. Thousands of medical professionals, scientific personnel, and technical experts served in internationalist missions, indicating that "medical diplomacy has been an effective foreign policy" (p. 6) tool for Cuba. In addition to the provision of medical resources to poorer nations, these policies generated powerful symbolism and went a long way in establishing Cuba's "medical prowess" (p. 183).

Feinsilver analyzes Cuba's health system in great detail, examining specific programs like the family doctor program, efforts to involve communities in medical assistance, a national network of polyclinics that facilitates services in rural areas, and specialized programs for women and children (pp. 34–38). Specific campaigns against sedentarism, smoking, AIDS, and other diseases are reviewed, as are relationships between health education and popular participation. She examines how the state allocates resources to the health sector, how priorites are established (often capriciously, due to Castro's intervention), and how scientific advances serve political aims.

Cuba's health system is highly centralized and entirely state run, needing a vast bureaucracy to maintain it. Thousands of medical professionals and bureaucrats (pp. 103–121) run and administer the system, expanding opportunities in health and medical fields for students interested in careers and service. In some sectors expansion has produced a surplus of specialists, allowing Cuba to "export" its personnel through lucrative overseas ventures. Finally, bureaucratization plagues the system and limits its flexibility.

State control over the health system facilitates the goal of a "health literate population" because "the government exercises control over the media, educational curricula and health services" (p. 64). Individual choices are severely limited insofar as there is only one provider—the state—but choice is not the principal value defining the system. In fact, though the quality of health services in Cuba is/was high compared to the typical developing country (pp. 91–103), the health system is integral to a development model in which the state is the only provider for and guarantor of "the people's health." This is the ultimate form of social therapy, converting the state into a compassionate doctor constantly worried about his patients' well being.

Massive resources from the former communist world subsidized Cuba's health system, while the state's control of the economy allowed for allocation of resources in a manner consistent with its goals. Castro's belief that his brand of socialism would be vindicated worldwide if Cuba's health indicators surpassed those of the advanced capitalist countries made him "the real Minister of Health," leading to impressive gains in infant mortality and life expectancy. Advanced programs in biotechnology, biomedical research and pharmaceutical exports (pp. 122–155) are reportedly Castro's pet projects in the 1990s.

"Healing the Masses" is putatively one the great achievements of revolutionary rule and is one (not entirely persuasive) explanation for the regime's survival in the wake of communism's collapse. The provision of health security produced the expected domestic political payoff, namely loyalty to the Revolution and support for a system of "collective goods." Abroad, the symbolism of a people "nourished, cared for, educated and fed" had its impact on ruling third world elites dissatisfied with high poverty levels often associated with dependent capitalism.

On the other hand, this approach institutionalized medical paternalism, sacrificing private initiative, choice, and freedom in exchange for medical security. For political reasons the system remains in place, but its effectiveness is severly compromised by shortages, incompetence, and demoralization among medical personnel. Consequently, Feinsilver concludes that in the 1990s, a lack of resources points to "a body politic that is ill" and directly affected by "a further deterioration in the Cuban people's health" (p. 211).

Two major omissions characterize this well-researched and well-written book that offers a critical contribution to Cuban Studies and to the literature on health and socioeconomic development. First, Feinsilver did not explore the vast differences found between the quality of care available "for the masses" and that provided for the elites. There is evidence that members of the military and political elites (particularly top leaders like the Castro brothers) have their own doctors, clinics, hospitals, and latest prescriptions for whatever ails them or their families, and immediate access to services when needed. In fact, entire wings and floors of some top hospitals like Hermanos Ameijeiras and others are reserved for the elite.

This is consistent with practices in former communist countries and raises moral and ethical questions about just how egalitarian Cuba's health system truly is. Finally, there is no mention of the documented abuses in psychiatry and other branches of psychology that treat political dissidence as a mental disorder, nor is the participation of state authorities and medical personnel in egregious abuses noted (see Brown and Lago 1991, *The Politics of Psychiatry in Revolutionary Cuba*).

In contrast, Dalton evaluates how Cuba's strategy of social development is shaped by classical interpretations of socialism as well as by ideological modifications stemming from the practical experience of the revolutionary leadership. He examines (1) "the conceptual underpinnings of the Cuban revolution" and how these framed a unique vision "of new men and women under socialism" (p. 2); (2) how the stabilization strategy of the 1970s and 1980s "could bring about the most effective conservation and utilization of human resources" (p. 3); (3) how complexity requires that professionals (social scientists, behavioral experts, psychologists) play increasing roles in policymaking (p. 3); and (4) how constraints "and social resistance continue to block the path to complete social reconstruction" (p. 4).

The basic assumptions are that human nature can be fundamentally reshaped through systematic and state controlled intervention in all spheres of life, and that the outcomes of such reconstruction represent a higher form of human development. In Cuba, these notions stem(med) from Ché Guevara's ideas on revolutionary morality, specifically from his belief in the perfectibility of human beings through coercive exhortation and moral rewards. A second source of ideological inspiration are Castro's fundamental views on struggle, consciousness, sacrifice, and if necessary, martyrdom (pp. 19–29).

That a purposive regime (which by Dalton's definition can only be of the disciplined, authoritarian, and socialist type) possesses the wisdom and will to recreate its subjects is seldom questioned. In other words, reconstruction is achievable if the masses cooperate and go along with the enlightened "vanguard." For instance, Castro's pronouncement "within the revolution, everything, outside the revolution, nothing" to intellectuals in 1961 served notice of what would and would not constitute permissible conduct and set the parameters for social deviation and individual compliance with revolutionary designs.

Values that deviated from the new political culture such as nonconformity, defiance, rebellion and most importantly, accountability, would be sacrificed on the altar of unity, obedience, passivity, and commitment. Mass organizations like the Committees for the Defense of the Revolution, the Women's Federation, labor unions, and the judicial system (particularly through the People's Courts) would be mobilized in support of revolutionary goals. These would be the building blocks of "the new society," one in which altruism would prevail over selfishness and competition.

The social sciences, particularly psychology, came under state control and work by Cuban psychologists contributed "to the capacity of the Cuban leadership to . . . mount a comprehensive and coordinated approach to human development and social planning" (p. 100). Dalton places great faith in the wisdom of Cuba's technocracy, maintaining that the contribution of sociologists and psychologists "does not consist of placating discontent but rather of enabling individuals to realize their full potential under socialism" (p. 99).

Simply stated, Cuba's strategy of social development aimed at nothing less than the remaking of individuals. Changing structures would be the first step in the process of recreation, that is, what would matter in the end would be the effectiveness of socialization. Much of the work of two generations of planners, psychologists, child development experts, and social engineers went towards that goal. Needless to say, Castro himself often guided these efforts, providing the intellectual justification that often turned untested theories into concrete projects.

Advances in psychology were used by the state in order to fight growing social problems like juvenile delinquency, absenteeism, and "poor revolutionary integration." Trained professionals delved into the causes of dissidence and political opposition and Castro himself "encouraged a revival of Freudian theories" in an attempt to get at the roots of social indiscipline and rising levels of criminality in the 1970s and 1980s (p. 76–77). It would have been politically incorrect to consider the fact that dissatisfaction with the strategy of coerced social development itself contributed to widespread anti-system behavior, because to recognize the conceptual failure would have undermined the proposition that the putatively enlightened vanguard had taken the wrong course.

While this is in some ways a very sophisticated analysis of the values and processes through which the Cuban leadership sought to transform a traditional culture into a socialist one, one also finds major flaws in the work. For instance, Guevara's credentials as a social psychologist are wildly overstated (pp. 22–24), nor are his colossal failures as a practitioner of guerrilla warfare assessed. And the pervasive decay evident in Cuban society demonstrates the superficiality of Guevara's thoughts on human development and "the new man." Thus most of his contribution to Cuba's strategy of social development needs to be reexamined. Second, some of the discussion on human rights and dissidence is shallow (pp. 77–81), characterized by such nonsense as "it is difficult to accurately gauge political dissidence in Cuba" (p. 78) for essentially two reasons: U.S. campaigns to discredit Cuba over the human rights issue and the regime's dismissal of all dissidents as counterrevolutionaries. Finally, childish observations about Castro himself, like the claim that "his personality is misunderstood" (p. 105) due to his "basic shyness with people"

(p. 105) take away some of the solid contributions this book does make.

Emory University JUAN M. DEL AGUILA

The Policy Challenge of Ethnic Diversity: Immigrant Politics in France and Switzerland. By Patrick Ireland. Cambridge: Harvard University Press, 1994. 327p. $52.00.

In this book, Patrick Ireland has taken on one of the most vexing problems in the history of democratic states: How should the state deal with members of a democratic society who are not full citizens? Ireland has a sophisticated and subtle research design that relies heavily upon comparative-historical analysis to address this question. Rather than relying exclusively on national case studies (of France and Switzerland), he has chosen to disaggregate national politics and policies by looking at the grassroots, or local, level. In addition to a review of immigrant politics in France and Switzerland from 1945 to the present, the reader will find detailed studies of immigrant organizations and political activities in two French cities (la Courneuve and Roubaix) and two Swiss cities (Schlieren and La Chaux-de-Fonds). This treatment of the problems of immigrant assimilation and political participation in Western Europe is highly original and insightful. The study extends and expands the pioneering work of earlier scholars of immigration and ethnicity, (e.g., Mark Miller, Gary Freeman, and Barbara Schmitter Heisler) yet, at the same time, offers us new insights into the political sociology of immigration.

In his efforts to understand the process of immigrant assimilation, Ireland brings to bear some of the newer literature on political participation, especially the literature on social movements, closely associated with the works of such scholars as Sidney Tarrow and Herbert Kitschelt. However, unlike the social movements literature, which draws heavily from class theories of politics, Ireland stresses the important role of the state and institutional structures in mobilizing new immigrant groups, often along ethnic lines. In effect, Ireland's thesis is that the state and institutional factors have been more important than class or ethnicity in structuring the political participation of immigrants in France and Switzerland. He is critical of the "Chicago School" of sociology, which places too much emphasis on ethnic or racial identity as the primary factors in immigrant mobilization. He argues that it is, instead, the state, through public policy, that shapes and creates the identity of immigrants. Likewise, he rejects, as "too theoretical and abstract," formal-legal theories of citizenship advanced by Rogers Brubaker, Yasmin Soysal, and others—because such theories, which stress national traditions such as *jus soli* and *jus sanguinis,* ultimately fail to tell us very much about integration, ethnic relations, or immigrant politics. Moreover, they lack rigorous, empirical tests.

Ireland proposes to bridge these theoretical and empirical gaps through his carefully crafted comparative case study of immigrant politics in France and Switzerland. He picked these cases because they "present the widest possible institutional variation," with France being a highly centralized, unitary state, and Switzerland, a decentralized, federal, or even confederal one. Yet because of the ethnic and socioeconomic similarities in the immigrant populations in France and Switzerland, Ireland is able to control for the effects of class and ethnicity on immigrant political participation and thereby isolate the institutional effects. In each case, he carefully reconstructs the history of interaction between immigrants and host-society institutions at the national and local levels. In France, we can see clearly how the French state slowly began to shift its policies away from demography and labor markets to a more integrationist and "rights-based" politics, allowing and even encouraging immigrants to organize in associations, often along ethnic lines. This was especially true during the long period of socialist rule in the 1980s, which saw the creation of many immigrant associations, such as France Plus and SOS-racisme. The "second generation" began to participate actively in political parties, while at the same time exerting new pressures on the republican state through overt displays of Islamic militancy and identity. Not surprisingly, the state and parties began to react to these pressures, either through accommodation (as happened in the scarf affair, where the socialist government of Michel Rocard decided to allow Moroccan girls to wear Islamic dress in school) or through rejection (as was the case with parties of the Right both in 1986 under Chirac and again in 1993 under Balladur, when efforts were made to stop immigration and slow rates of naturalization). The broader point in Ireland's historical analysis is that institutions were shaping immigrant politics, leading to a showdown between the authorities of the republican state and an increasingly assertive "second generation."

The smaller, multiethnic, and confederal Swiss state responded very differently to the pressures of immigration. Overall policy was driven strictly by labor shortages and the desire to maintain rotation of foreigners into and out of the labor market. Any consideration of integration (for those foreigners who managed to obtain rights of residence) has been confined largely to the local or cantonal level, despite periodic attempts to crack down on rising levels of immigration, through the *Uberfremdung* initiatives (basically national referenda calling for a halt to further immigration). But it was precisely the very local nature of Swiss direct democracy that gave foreigners many institutional channels through which to organize and press their demands on local and national/federal authorities. As was the case in France, Ireland finds that foreigners were able to mobilize and to use the institutional channels available to them to express their interests. At the same time, these institutions were shaping immigrant identities, especially among the second generations. The more detailed, idiographic studies of the two French and two Swiss cities simply reinforce Ireland's thesis that institutions determine the political opportunity structures, which, in turn, shape immigrant politics in the two countries.

While this book represents the best scholarship on the political rights and participation of immigrants in two democracies, it does not deal with other equally important dimensions of citizenship, namely, civil rights (e.g., guarantees of due process and equality before the law) and social rights (access to such basic social services as education and health care). Ireland seems so determined to make the point that "institutions and politics really matter" that he overlooks these other crucial areas of citizenship, which are at the very heart of the debates over immigration and ethnicity in the United States and

Western Europe. Perhaps this is because Ireland assumes the primacy of political rights over civil and social rights. But if we go back to T. H. Marshall's original typology of rights (which the author mentions only in his conclusion), membership and citizenship in a liberal society are contingent first and foremost on civil rights, or basic guarantees of protection of the individual from the (police) powers of the state. On the other hand, in a liberal democracy, rights are a function of political struggle, and Ireland's book makes this point exceptionally well. His findings indicate that ultimately the French state, with its Jacobin emphasis on broader "human rights, equality, and *laicite*," is better equipped to handle the challenge of immigration and ethnic diversity, than the Swiss state, which lacks a "defining ideology" and is "out of sync with Europe." His assumption is that the universalistic, French republican model is a better model for Europe, than the particularistic, Swiss model of democratic localism.

Whatever criticisms one may have, this book should be required reading for anyone interested in the politics of immigration and ethnicity, two of the most important issues facing liberal societies in an increasingly interdependent world.

Auburn University JAMES F. HOLLIFIELD

Chasing Progress in the Irish Republic: Ideology, Democracy and Dependent Development. By John Kurt Jacobsen. New York: Cambridge University Press, 1994. 226p. $59.95 cloth, $18.95 paper.

In *Chasing Progress*, Jacobsen argues that development policy in the autonomous Irish Free State after 1922 and the independent Republic of Ireland after 1949 provides a test of the importance of ideology in shaping development outcomes. Jacobsen critiques simple dependency theory for arguing that dependent economies respond automatically to shifts in the global economy. Like Peter Evans's *Dependent Development* (1979), *Chasing Progress* argues that state policy matters and that because it matters, ideologies will influence what states perceive as their menu of policy options, their choice from that menu, and their ability to sell policies to the public, particularly (as in the Irish case) a democracy with a broad franchise.

In Ireland, development policy in the 1920s concentrated on an orthodox pursuit of comparative advantage in agricultural exports, under the tutelage of British-educated, free-trade-minded civil servants supported by farmers. Policy shifted to import-substitution industrialization and autarky in the period 1932–58, as a development-minded Fianna Fail party tried to create a class of manufacturers to support itself. As this (inevitably?) gave out, policy turned to an export-led growth policy that paid multinationals to relocate labor-intense parts of their production process to Ireland. Accession to the European Community in 1972 reinforced this policy, but the emergence of similar and often cheaper sites for outward processing forced the state to borrow heavily abroad to subsidize multinational corporate investment and to press wages downward through quasi-corporatist bargaining. The left parties accepted this in the hope that multinational corporations would create a working class to serve as *their* social base. But in the late 1980s, rising debt-service costs forced cuts in social spending and destroyed emerging corporatist institutions. Instead, the absence of any institutions to support alternative development strategies reinforced by neoliberal ideologies led Ireland to persist with its old, multinational-corporation-led strategy.

Chasing Progress adds four things to the literature on development: (1) it carefully delineates the content of, and shifts in, Irish development policy, adding historical depth to the literature on Ireland, as well as more European data to a development literature that usually centers on "southern" cases; (2) Jacobsen's study also suggests that the proliferation of export-processing zones (which often fail to create any linkages to the rest of the local economy) make them a weak tool for development; and it highlights the importance of ideas and institutions in (3) policy selection and (4) execution. Ireland's failure to create local linkages—in sharp contrast to the Asian newly industrialized countries—stemmed from the predominance of orthodox economics in policy circles and the parallel resistance of landed and mercantile elites to the creation of any *dirigiste* planning agency. In turn, workers' willingness to accept liberal and neoliberal policies occluded the construction of more *dirigiste* agencies that might have created the countervailing ideas, industrial development, and tighter labor markets, which then could have served as the basis for a stronger labor movement.

This said, *Chasing Progress* has four great weaknesses. First, the last third of the book is written in a chronological, almost journalistic style that ignores the analytic framework Jacobsen lays out at the beginning. Jacobsen's failure to do a little in the way of comparison with other similar cases or counterfactual cases exacerbates this flaw. More comparison could have told us more about the realistic policy options for small economies, and how export-processing zones can be used for development. Jacobsen argues that a closed, Keynesian economy was a better policy option for Ireland. Comparison with New Zealand, a very similar agricultural exporter, shows the limits to this policy. New Zealand achieved substantial levels of industrialization in 1935–75 with this policy. But by the 1970s, the policy was bankrupt, and by the 1980s, so was New Zealand (like Ireland). On the other hand, both Hong Kong and Singapore have achieved substantial and enduring industrialization as export-processing platforms, successfully upgrading skills and wages in manufacturing, despite multinational corporations' predominant role in their economies.

Second, the book lacks any systematic analysis of the political economy of Ireland. It lacks a full analysis of what Ireland produces and exports and, in particular, does not show how important multinational corporations were for the economy, in what sectors, and with what consequences for the balance of payments. Most disturbingly, for a Marxist-type account, it does not show where the money went: Who holds the billions of dollars of public debt that subsidized multinational corporate investment? A local rentier class? Foreigners? Pension funds? All this matters for an understanding of class interests and policy preferences.

Third, this in turn makes the discussion of ideological influences on policy virtually unintelligible. Ideas rarely have an independent force of their own; social groups put ideas forward. But without an understanding of social groups' relative position in the economy, their (lack of) access to policymaking institutions, and their ability to back their positions with cash or collective action, we cannot understand whether a real clash of ideas occurred or whether ideas simply provided a window dressing for decisions taken elsewhere.

Finally, this book is based on Jacobsen's 1982 dissertation and consequently reflects the debates of the 1970s, which investigated whether state policy could overcome or modify dependency. But more recent literature, like Robert Wade's *Governing the Market* (1990) or Stephan Haggard's *Pathways from the Periphery* (1990) has already gone several steps further, looking at the coalitional bases for specific institutions and policies, at the institutional context for policy formation, and at how state policy actually affects firms' behavior. Little of this can be found in Jacobsen's book. In this sense, Jacobsen, like Ireland, is still chasing progress.

University of Virginia HERMAN M. SCHWARTZ

Reversed Realities: Gender Hierarchies in Development Thought. Naila Kabeer. London: Verso, 1994. 346p. $64.95 cloth, $18.95 paper.

Naila Kabeer has collected into one volume a group of her papers that comprise a feminist critique of mainstream social science, especially economic development theory. Since the chapters were written at different times and in response to different demands made upon the author, they do not necessarily have to be read in sequence, except for the first three chapters each of which builds upon the preceding chapter. The author is at pains to explain the format of her book in the preface and the reader is well advised to read it carefully. The first four chapters take up the broad question of the relation of feminism to development theory, or perhaps more bluntly, why women are systematically left out of mainstream development equations. The next three chapters examine concepts in development thought and practice. These include the liberal economic premise of altruism in family economic practice and the question of poverty and poverty measurement as both relate to women. The last three chapters focus on three specific areas of development practice that involve women: population policy, female empowerment, and the political context of gender training frameworks. These chapters investigate the concepts and strategies of the planning agencies themselves, including their subtle bias against women that vitiates the very results these agencies hope to achieve. The book concludes with the need not only for radical change in mainstream economic thinking about development, but also for the transformation of the institutions that determine development policy. The whole book represents a cogent and persuasive argument that unless development policy and practices are changed to include women as a necessary object of development strategy, development will continue to elude those countries seeking to achieve it. At best they will achieve market production at the cost of mass impoverishment and social breakdown.

This is a powerful book. What strikes the reviewer most is that the author attacks development theories and the concept of development at their very roots, laying open the assumptions on which the theories rest. Liberal economic theory, she argues, is based on the operation of individual units. All agents within society are motivated by one and the same goal, to maximize their individual utilities. Maximization requires allocating limited means to competing ends. Success in allocation is termed rational behavior. Individual behavior is then aggregated into the family and into the state with each different level of analysis being driven by the same principle, the maximization of individual utilities. Modernization and development theory evolved directly from liberal economic theory. Since all individuals are characterized equally by the same behavior, then the process of economic development from traditional society to an industrialized society must trace a similar path in all societies.

Kabeer contests these assumptions. Gender inequities are built into the structure of families and into national or international government hierarchies. Liberal economic theory as well as classic modernization theory are virtually silent about women. Women do not exist. The individual unit is the male, who is assumed to be the benevolent dictator of his family. The male controls what passes for knowledge in society as well as the levers of power. In the 1970s, the Women in Development (WiD) approach attempted to correct the lacunae on women. The focus was on making women and women's problems in development visible to the policy makers. One tangible result of this approach was the inclusion of women as a chapter or a section in development planning, but Kabeer thinks that WiD at its most fundamental level, essentially agrees with the liberal economic model. What is needed, in her view, is an entirely new model based on new modes of thinking. This model must not just relate means to ends, it must posit the end of development in terms of the well-being of all members of society, not simply the maximization of individual (family, state) utilities. In her excellent chapter on population policy, Kabeer argues that maternal health and well-being require attention to *"women's status"* (italics mine), long before a woman becomes pregnant. A population policy that focuses on birth control without attention to the other facets of a woman's life, risks backfiring on the policy planners. Rather than maternal health, the aim of international health agencies should be women's health, in all its dimensions.

This book should be required reading for participants preparing for the forthcoming Women's Conference in Beijing in August 1995. It should read by UN and other international agencies. In making her argument, the author brings together a wide composite of feminist and development literature, as well as her own considerable experience in the field. The book should be read with two caveats in mind. The first is that the author attacks conventional development theory from the standpoint of women's needs and roles in the so-called Third World. Excluded totally from the study are references to women in the post-communist world, many of whose needs are similar or who were specific victims of male-dominated economic development. Second, also absent and most sorely missed from this admirable critique of development thought is the interrelationship of women's nonexistent role in development theory and environmental degradation. While the author refers to and approves the concept of sustainability, she does not address the implications of improving women's status on environmental amelioration. Her book can provide important theoretical background for those of us dedicated to this endeavor.

State University of New York, Brockport BARBARA JANCAR-WEBSTER

The Power of Symbols against the Symbols of Power: The Rise of Solidarity and the Fall of State Socialism in Poland. By Jan Kubik. University Park: Pennsylvania State University Press, 1994. 322p. $42.50 cloth, $14.95 paper.

The year 1989 was a great triumph for humanity but something of an embarassment for the social sciences. Not many people foresaw its possibility. Of course, no one predicted it exactly, including the precise timing. The social sciences obviously are not in *this* kind of prediction business. But even as events were unfolding before our eyes, they were hard to explain. They did not fit our categories and were not allowed by our axioms. Perhaps more surprisingly, not much has changed since 1989 in the social sciences. There has not been a profound rethinking of our basic categories. "Those poor people who wrote and taught about communism," we say to each other. "They sure got it wrong; they must really be embarassed." But in fact, the communism scholars among us were simply applying to their special area of expertise the same ideas we all routinely traffic in. All of us should be embarassed.

Why was 1989 such a surprise both in the social sciences, and for the world at large? A big part of the answer is that the world—and the social sciences within it—is dominated by cynics. And cynics are simply not equipped to understand the revolutions of 1989. The crucial lesson of the collapse of communism for the social sciences lies in its forcing us away from the various versions of cynicism that have dominated our theories. Poland is a crucial empirical case. And Kubik's book, with its analyses of symbols and rituals borrowing heavily from cultural anthropologists such as Victor Turner, provides some very helpful intellectual instruments.

We have accumulated already a very substantial literature in many languages on the Polish trade union/self-limiting social movement Solidarity and its role in the collapse of communism in Poland. Jan Kubik's book adds to it the perspective of a political-cultural anthropologist. The book is primarily about the culture, the ceremonials, and the symbols of Solidarity and their role as legitimating devices and hence as instruments of power. The author spent the early 1980s in the department of social anthropology at the Jagiellonian University in Cracow, and then in the department of anthropology at Columbia. The book is a revised version of his Columbia dissertation.

The author is, in various ways, both an insider and an outsider in relation to his subject. He is Polish and has experienced directly the symbolic power and political appeal of Solidarity. But he is writing from the distance of an American university and in the language of a discipline especially adapted to cultural analysis by outsiders. In addition, much of the book is about Catholic culture and symbolism, while the author is a Protestant. Altogether, the author begins his task with many strikes in his favor. The result is a book that will be obligatory reading for anyone who wants to understand Solidarity, or, for that matter, the collapse of Communism.

The book purports to answer three related questions: How can we explain the extraordinary forms of protest in Poland, compared to those in other communist countries? Why did Solidarity happen in Poland? Why did it survive the period of martial law and the decade of repression that followed December 13, 1981? (p. 6). The book does provide (partial) answers to those questions, but it systematically focuses only on one aspect of those answers. Its main source of insight is cultural anthropology, and it does not have much to say about anything that does not interest cultural anthropologists. For example, it barely mentions the economic collapse of the communist system in Poland, but it provides detailed analyses of the Pope's trip to Poland in 1978, the ceremonies and rituals of the opposition during the late 1970s, the "romantic" culture (symbols, ritual) of the strikes in August 1980 (from which Solidarity emerged), and the culture and symbols of the union itself.

I recommend the book highly but not because it fully or systematically answers these three questions. It does something perhaps more important, precisely because it allows us to see in such detail and depth *one* aspect of the Polish situation as communism was collapsing.

Legitimacy is the key concept of Kubik's book, and it is the key to understanding what the collapse of communism can teach us. The events surrounding 1989 show us the power of legitimacy. They show how weak those who lack it can be, no matter how many carrots and sticks they can wield. And these events also show how powerful those who have legitimacy can be, even if they have nothing else. We need to incorporate a detailed and systematic science of legitimacy into the detailed and systematic understanding of domestic and international realpolitik we already have. There are, of course, precedents to build on here but also a great need for the building itself.

Here, Kubik's book makes an important contribution. It is best seen, I think, as a detailed account of the operation of one aspect of legitimacy—the sacred one—in the context of the collapse of communism in Poland. The anthropological literature on which Kubik draws identifies legitimacy with *the sacred* (broadly conceived). And Kubik's book shows how what Polish society holds sacred (its fundamental values, its religious and patriotic symbols) defeated the tanks and prisons controlled by the communist regime. Legitimacy is certainly not exclusively based on the sacred (rationality must be also considered), but it is certainly *in large part* based on the sacred. And the sacred dimension of politics has not been much analyzed, certainly not as deeply as it is analyzed here.

University of Maryland, College Park KAROL SOŁTAN

Stopping the Killing: How Civil Wars End. Edited by Roy Licklider. New York: New York University Press, 1993. 354p. $50.00.

It doesn't take a specialist to resonate to the concerns of this work. Confronted with fratricidal agonies like those of Bosnia, Sudan, Sri Lanka, or any number of other current civil wars, nearly anyone will ask, "Mustn't there be an alternative to such horrendous violence as a means for settling political conflicts?" This book reinforces the conviction that such alternatives are easily envisaged—by outsiders. But whether any insight yielded by social science is apt to help realize those possibilities is altogether more problematic.

The essays in this book grow out of a project on Civil War Termination, begun in 1987 and sponsored by several institutions, including the United States Institute for Peace. Seven of the 13 chapters are case studies; the balance are theoretical essays. "The major issue," the editor notes, was to explore "why some civil wars end so that large-scale civil violence is not resumed" (p. 13). The collaborators agreed to weigh the roles of the following considerations: (1) the nature of the issues underlying conflicts, (2) the internal politics of each side, (3) the military balance in the field, (4) the role played by outside parties, and (5) the effect of the polity emerging from civil conflict on the stability of its resolution. Close attention to these variables, it was hoped, would disclose parallels in the successful resolution of civil war.

It would be hard to identify a salient theoretical conclusion emerging from the work. All in all, the case studies strike one as more informative than the theoretical essays. Careful analysis of the nuts and bolts of individual cases leaves one with the sense of grasping the forces and circumstances that accounted for each particular outcome at the particular historical moment under analysis. But efforts to identify significant patterns in these chains of events or to anticipate how similar antecedents might lead to similar results in other instances are more labored.

Consider, for example, Stephen John Stedman's analysis of the resolution of Zimbabwe's civil war. Here, it seems clear, the aggressive role of the British in cajoling and threatening the various parties and playing them off against one another was decisive in shaping the ultimate solution. But this finding offers little precedent for the other cases. In some (e.g., Sudan, as documented by Donald Rothchild and Caroline Hartzell), outsiders determined to broker the peace played at most a mildly facilitative role. In others (e.g., Nigeria, as depicted in James O'Connell's essay), strenuous efforts by would-be mediators seemed all but irrelevant to the ultimate solution.

In light of such diverse findings, the theoretical studies take on an *on the one hand/on the other hand* quality. Probably it would be wrong to blame the authors of these chapters for the scarcity of strong positions here. The cases making up the universe of civil wars may simply be so heterogeneous that similarities (if indeed there are any) do not stand out in a sample as small as seven. Perhaps our salient moral reaction to civil wars (i.e., as a scourge) seduces us to an unwarranted belief that the processes by which their destructive processes begin and end are somehow of a piece.

One illustration of the elusiveness of parallels is evident in the attempt to map a salient theoretical concept—I. William Zartman's notion of a *hurting stalemate*—onto the case studies. This is the idea that "each side is willing to change policies only if it believes that it cannot achieve its desired ends by violence at a tolerable cost and that its relative position will decline in the future. The hypothesis would be that a stable agreement is possible only if *both* sides believe this at the same time" (p. 16). The idea is eminently plausible. But editor Roy Licklider puts his finger on the matter in his concluding chapter, when he writes that the concept is deceptively hard to work with. "*Stalemate* can be defined without undue difficulty but will not produce a settlement by itself; both sides must *perceive* the current situation as untenable and unlikely to improve *in the future*. . . . The concept is difficult to operationalize without being tautological; we want to know if a hurting stalemate exists in order to predict whether or not the parties will alter their policies, but we only know if it exists if the parties do alter their policies." (p. 309).

Licklider's acute observation could be extended to the broad genre of attempts to apply rational-choice analysis and other calculative models to conditions faced by political actors in life-and-death struggles. Perforce, a key term in all such analyses must be the costs or rewards of pursuing a particular political objective, *as perceived by the actors themselves.* The problem here is not so much that these costs are subjectively defined but rather that *these definitions themselves are continually emergent;* that is, they are likely to undergo continuing redefinition in light of developments both exterior to the hearts and minds of the actors and strictly internal to them.

Does all of this mean that there are no meaningful parallels to be found in the processes leading to the conclusion of civil wars? That conclusion would be premature. The best chance of identifying such patterns might be to plot and compare the scenarios of much larger numbers of such conflicts, in the hopes that "family resemblances" would emerge within significant subsets of the cases.

Until such a more ambitious analysis can be carried out, these essays serve to remind us just how heterogeneous the world of civil conflict really is.

State University of New York, Stony Brook JAMES B. RULE

Electoral Systems and Party Systems: A Study of Twenty-Seven Democracies, 1945–1990. By Arend Lijphart. New York: Oxford University Press, 1994. 209p. $29.95.

This new book by Arend Lijphart combines the virtues of being both an invaluable reference source and simultaneously a new advancement in the comparative analysis of electoral systems. Called by the author himself a "collaborated project with as its product, a single-authored book," *Electoral Systems and Party Systems* has benefited from the indepth knowledge of "experts on each of the twenty-seven countries" and gives the most extensive, detailed, and accurate account so far of rules of legislative elections and cases of institutional change for such a broad collection of stable democracies (pp. 1, 8). Information is presented in a very compact form in the text of the book, numerous tables, and appendices. In addition, the author makes his full data set available to those interested in replication and alternative research. Undoubtedly, the book will become a standard reference in the field. However, the value of Lijphart's book as a source of reference should not obscure its important input to the methodology of institutional analysis.

At least in three major ways, this new book contributes to the studies of electoral institutions. First of all, it offers a reasonable definition of a single institutional measure, effective threshold for legislative representation, to describe and classify all existing electoral systems. Second, the author adds the rules of Euro-elections to the conventional collection of the rules of legislative elections in stable democracies. This addition not only increases the number of cases under consideration but also opens an opportunity for extensive controlled comparisons where consequences of institutional change can be observed directly—within the same country-specific context. The third important contribution that the book makes to the methodology of comparative research is that in order to derive substantive conclusions, it successfully combines country-specific controlled comparisons with the larger-sample cross-country data analysis.

The main difficulty of assessing the influence of institutional parameters on the performance of political systems is the multiplicity of dimensions of difference among the electoral systems while the number of available cases is limited. Lijphart combines several key institutional variables into a single indicator that is meant to capture much of the variety of institutional impacts. Magnitude of electoral districts, legal thresholds for legislative representation, and, to a certain degree, electoral formula, as well—all are melted into what the author calls an *effective threshold.* It would be nice, of course, to have even more institutional characteristics combined into a single indicator. This, however, is not possible, as it would require ad hoc assumptions about the directions in which each included parameter impacts on party systems. In fact, even combining the original three parameters into the effective threshold had as its prerequisite both analytical and empirical knowledge of these parameters' separate influences. Fortunately, not only are the effects of the three parameters within the effective threshold relatively easy to hypothesize, but also, a substantial volume of literature studying these separate effects has been accumulated, beginning with Duverger's seminal *Political Parties* (1954), followed by Rae's *Political Consequences of Electoral Laws* (1967), Taagapera and Shugart's *Seats and Votes* (1989), and Lijphart's *Democracies* (1984). Thus, Lijphart's effective threshold is not likely to arouse a controversy, as there is basic agreement on all of its building components, although the reader may want to see the assumptions on which the definition of the measure is based: (1) that the number of parties equals the magnitude of the district and (2) that there is no variation in parties' support across electoral districts (discussed at greater length).

The author also analyzes the impact of other major institutional features alongside the effective threshold; the separate roles of electoral formula and assembly size are examined. However, the influence of the composite effective threshold remains the strongest both in controlled and cross-country comparisons. Once general connections between the effective threshold, electoral formula, and assembly size and the resulting features of party systems are established, the author can move on to studying the additional impact of other institutional parameters (e.g., the ballot structure, vote transfers, malapportionment, and presidency) that may generate additional incentives in party competition.

The dependent variables Lijphart seeks to explain by institutional parameters are disproportionality between parties' electoral and legislative strengths, multipartism, and the occurrence of legislative majorities. With respect to the first of these, his conclusion is that "the degree of electoral disproportionality or proportionality responds very sensitively to the rules of the electoral system," as "two-thirds of the variance in disproportionality is explained by the electoral system" (pp. 139, 140). Electoral system also explains "as much as 44 percent of the variance" in the frequency of parliamentary and manufactured majorities, where "the effective threshold does most of the explaining by itself" (p. 142). But there are conclusions with which some readers may strongly disagree. For example, in his final chapter, which offers practical advice on the use of electoral systems, Lijphart asserts: "For electoral engineers the electoral system is not as strong an instrument for shaping the party system. The influence of electoral system on the effective number of elective parties is especially weak" (p. 141). Lijphart arrives at this conclusion by assessing the independent institutional influence without taking into consideration any of the noninstitutional factors that may be of importance and that may simultaneously exert counteracting pressures. Moreover, it would not be unreasonable to hypothesize that in many cases, specific electoral institutions are chosen precisely in order to *balance* those other influences. After all, hardly anyone would argue that similar sets of rules must always produce identical results and that if we observe different outcomes then the rules are inconsequential. Clearly, in some electorates, initial conditions (e.g., linguistic, religious, or class cleavages) create extra pressures in the direction of party multiplication. Thus, the same number of "elective" or "legislative" parties may correspond, in one country, to a success in the institutional reduction of the number of parties, while resulting, in another country, from an effort to institutionally promote "representativeness" and support smaller parties. In both cases, then, the rules of elections would matter greatly, but the researcher who ignores the noninstitutional differences between these two hypothetical countries would remain under an erroneous impression that institutions are of no consequence in either of them.

This book will not resolve all the disagreements in the studies of electoral systems and their role in shaping party systems. But it was not intended by the author to do so. As the author states in the introduction, "a host of important decisions must be made with regard to classification, measurement, and other methodological matters"; correspondingly, he wants "to make it as easy as possible for . . . readers to reanalyze the data according to the alternatives that *they* prefer" (p. 8).

Washington University Olga Shvetsova

The Failure of Presidential Democracy. Edited by Juan J. Linz and Arturo Valenzuela. Baltimore: Johns Hopkins University Press, 1994. 436p. $65.00. Paper version in two volumes: **Comparative Perspectives,** $13.95 and **The Case of Latin America,** $16.95.

This is an impressive book. It contains an extended, fleshed out, and nuanced version of the argument by Juan Linz that parliamentary systems are better able to cope with societal conflict and crisis than are presidential; additional contributions to the general debate over which form of executive works better by Giovanni Sartori and Arend Lijphart; a preliminary examination of the empirical evidence that parliamentarism increases democratic stability by Alfred Stepan and Cindy Skach; and case studies of the effects of presidentialism (or semi-presidentialism) in France and a number of Latin American countries.

Linz argues that presidentialism tends to lead to political instability for two reasons. First, presidents who, for whatever reason, have "failed" cannot be legally replaced until their terms have ended, which creates support for illegal seizures of power during crises. And second, presidentialism permits the development of intense conflict between the executive and legislature, which cannot occur in parliamentary systems. Such conflict has preceded democratic breakdown in a number of countries. Versions of this argument, and of Valenzuela's convergent one focused on the Chilean case, have been circulating for about ten years, vying for that most coveted prize in political science, Most Widely Read Unpublished Paper, and short versions have appeared in print before. Readers already familiar with the bald statement of the argument above should not, however, be deterred from reading the one in this book. It contains more interesting detail, more examples, and more nuance, and is therefore more persuasive than earlier statements of the argument.

To my mind, however, given the prior circulation of Linz's argument, the most useful and intriguing part of the book is the set of case studies on the actual effects of presidentialism in a number of Latin American countries. These studies, most of them done by scholars outstanding for their country expertise who have not previously focused squarely on institutional issues, begin to fill what has been a glaring hole in the literature. Until recently (and with a few notable exceptions), those with the in-depth knowledge of individual countries needed to understand the way their institutions really work have paid little attention to them, while those interested in institutions have tended to lack sufficient country expertise to make really persuasive arguments. The contribution of these case studies lies not so much in their "tests" of the Linz–Valenzuela argument as in their demonstrations of the way differences in political institutions really affect the handling of events and crises in different contexts.

All of the Latin American case studies in this volume are very good, and several are much more than that. "Party Politics and the Crisis of Presidentialism in Chile" by Arturo Valenzuela is a gem. If someone urgently needed to understand recent Chilean political history and could read only one thing, this is the one I would recommend. Michael Coppedge's innovative and careful delineation of the interaction between intense party discipline and presidentialism will give pause to all of us who, with Brazil or Ecuador at the top of the mind, have tended to see party discipline as a panacea.

While the Latin American case studies, along with the Stepan and Skach article, serve as empirical tests of the theoretical arguments advanced by Linz and Lijphart at the beginning of the book, the conceptual status of the French case is less clear. In "Presidentialism and Political Stability in France," Ezra Suleiman shows the improvement in government performance in France that followed the transition from a parliamentary to a semi-presidential system. This experience could be read as a challenge to the Linz–Valenzuela argument. Sartori certainly takes this position, arguing that pure parliamentarism tends to lead to unstable, ineffective government, even if it is conducive to the maintenance of democracy. The French case is not by itself very persuasive evidence, however, since party behavior has changed radically in France since 1958, greatly affecting government performance. And the change in party behavior can probably be attributed to changes in electoral rules that occurred at the same time as the change in form of the executive, more than to the change in the executive itself.

An alternative view of French experience is that it demonstrates that a hybrid system—the kind that has actually been chosen in most former communist countries and seems much more likely than a pure parliamentary system to be chosen in Latin American countries—can under some circumstances work well. As Bolívar Lamounier's article on the trials and tribulations of the campaign for parliamentarism in Brazil shows, major obstacles impede the transition from presidentialism to parliamentarism. Most ordinary people in countries lacking a parliamentary tradition identify voting for the country's most important leader with democracy itself. Furthermore, political leaders who harbor presidential ambitions, and their followers who can expect to benefit from their leader's future good fortune, cannot be expected to spearhead transitions to parliamentarism. Given these interests and attitudes, it seems unrealistic to expect many transitions to pure parliamentarism to occur. In *Presidents and Assemblies* (1992), Matthew Shugart and John Carey have shown that, in contrast to the French experience, hybrid systems often work badly. The time seems to have come for scholars, who, like revolutionaries, must sometimes run to keep up with the masses, to turn their attention to trying to understand the institutional details that affect whether hybrid systems work well or not.

The debate over the relative merits of parliamentarism versus presidentialism has helped bring institutions back in. We now understand the effects of presidentialism much better than we did ten years ago. As a result, the research frontier has shifted. As all the case studies in this book demonstrate, other institutional features interact with the form of the executive to jointly determine outcomes. I hope that *The Failure of Presidential Democracy* is the harbinger of a whole spate of future research on the effects of less-studied political institutions.

University of California, Los Angeles BARBARA GEDDES

Central Asia and the Caucasus after the Soviet Union: Domestic and International Dynamics. Edited by Mohiaddin Mesbahi. Gainesville, FL: University Press of Florida, 1994. 344p. $49.95 cloth, $24.95 paper.

The collapse of communism caught most Sovietologists unawares, and then the rapid disintegration of the Soviet empire left the profession unprepared to deal with the plethora of new independent states. This was especially true of the southern republics in the Caucasus and in Central Asia. The combination of an unprecedented situation and the general lack of expertise was in part responsible for the quick succession of facile scenarios about the future: the Great Game competition, the Turkish/Iranian models, the domino-effect theory. Alongside these, a few analyses that are less black-and-white have also managed to appear. The volume edited by Mohiaddin Mesbahi belongs to the last category. It comprises papers originally given at an international conference sponsored by the Iranian Institute for Political and International Studies and held in Teheran in March 1992.

Several features distinguish this collection of 14 essays from the many others that have appeared in the past three or four years. The book provides insights of experts from several social science disciplines, that is, not just political scientists but also historians and anthropologists. The geographic-national representation is equally diverse, with scholars from the United States, Western Europe, Russia, and Central Asia listed on the contents page. It is nicely balanced between discussions of the theoretical issues raised by geopolitical change and of the distinctive situations in the individual countries. It avoids dwelling on Soviet failures and predicting sharp breaks—a trend that became quite fashionable in the West during the late 1980s. Instead, many of the contributors accept and analyze the symbiosis that had formed during the years of Russian and Soviet rule. Accordingly, the predominant message is that in the foreseeable future, attention should be paid more to continuities than to discontinuities.

That tone is especially evident in one contribution to the opening section of the volume, which is devoted to ethnicity and Islam. M. N. Shahrani's paper on Soviet legacies to Muslim Central Asia makes the point that the local cultural and attitudinal resistance to Soviet efforts at modernization had not been as deep as many Western experts have argued. To be sure, Soviet developmental policies had negative consequences on the peoples and cultures of the region, but their impact and persistence cannot be ignored. Various political and economic developments in the past two years confirm Shahrani's insight, for they indicate a gap between the new modernizing paradigm proposed by the democratic reformers in Moscow and the persistence of old authoritarian preferences among the political elites of Central Asia.

The next two parts cover the new Muslim states in the western regions of the former USSR (the autonomous republics of Tatarstan and of the North Caucasus and Azerbaijan) and the five independent republics of Central Asia, respectively. Most of the contributions are descriptive and judiciously balanced in their coverage of the political, economic, and ethnic developments in the Soviet and post-Soviet periods. Marie B. Broxup's contribution on the Muslim entities remaining within the Russian Federated Republic stands apart with its stress on the unremitting Russian-Soviet oppression since the conquest of Kazan in 1552 and its insistence on a basic unity of the entire area (i.e., from Tatarstan to Dagestan and beyond the Caspian Sea), a unity claimed to have survived intact from olden times.

The fourth and last section deals with the new geopolitics of the former Soviet South. Milan Hauner sets the debates on Russia's cultural identity and strategic destiny—the persistent ambivalence caused by the state's Eurasian nature—into an informative historical perspective. And a fine essay by the book's editor nicely combines a detailed discussion of the emergent redefinitions of Russian interests in the area with a deft analysis of the changing strategic picture.

Altogether, as far as collective volumes go, this one (though obviously not definitive in a situation that is extremely fluid) provides readers with many worthwhile (far from pat and quick) historical and analytical insights.

Columbia University ELIZABETH KRIDL VALKENIER

State Power and Social Forces: Domination and Transformation in the Third World. Edited by Joel S. Migdal, Atul Kohli, and Vivienne Shue. New York: Cambridge University Press, 1994. 333p. $59.95 cloth, $19.95 paper.

This edited volume by a group of leading scholars seeks to provide a more pertinent and fruitful comparative theory of the state by bringing society back into the picture. The principal editor expresses dissatisfaction with received state theory, which assigns to the state "an [erroneous] ontological status that has lifted it apart from the rest of society." The contributors are committed to a "new anthropology of the state" in which the interactive struggles for domination between state and society and the social changes involved in such struggles are again placed at the center of analysis. The study is thus an attempt to recover the political sociology of the state and to build anew on those theoretical foundations.

The book contains provocative essays on a variety of state-society topics: traditional politics in Brazil, state power in China, Indian democracy, ruling classes in Africa, labor and state in China, business and the Egyptian state, social change in the Ottoman Empire, peasant-state relations in Africa, associational life in Africa, and, as well, general essays on the state-in-society approach. For anyone interested in knowing where the comparative theory of the state might be heading at the present time, this volume provides some excellent signposts. The case studies are particularly rewarding and enlightening for the many excellent insights and reflections they contain on the cases addressed. The book runs into the difficulty of achieving consistency between the general theory and the case studies, but few edited volumes in political science surmount that obstacle.

A more worrying problem, however, is the theory itself. States are differentiated from social forces presumably with the aim of exploring their interactions, but conceptual ambiguity soon sets in. *Social forces,* as a concept, turns out to be a very broad category encompassing almost anything having to do with social action and social relations. Perhaps even that can be justified at least up to a point: aggregative concepts have their merits. At one point, however, even the state is referred to as a social force (p. 30). From that point onward, the reader is in doubt about the theoretical coherence of the enterprise.

The volume quite rightly draws the reader's attention (p. 11) to the similarities between state-society struggles in sixteenth- and seventeenth-century Europe and comparable Third World struggles today. The European struggle was fundamentally about state sovereignty: establishing the supreme authority of a government over a particular bounded territory and its population, and establishing its independence of all other such governments. That project of building the independent nation-state arguably is the greatest political project of modern history—at least, up to the present time, when the endeavor to go beyond the nation-state may be underway. In Europe that project to build nation-states often involved war as a historical arbiter, as E. L. Jones and Charles Tilly make clear. Unfortunately, this fundamentally important theme, alluded to at the beginning, is left undeveloped. While there are many index entries for "state," "civil society," and "society," there is but one entry for "sovereignty" and none for "war," "state system," "territory," or "citizenship."

We really cannot fully capture the state in theory without recognizing that it is Janus-faced: it faces both inward at domestic society and outward at international society. International relations theory can often be faulted for failing to acknowledge the inward facet of the state, but comparative theory can likewise be taken to task for just as often failing to take on board the outward facet of the state. Unfortunately, this volume can be criticized for exhibiting the latter. However, if I have got it wrong and the research project of comparative state theory divorced from international relations theory does make sense, then this volume probably represents the best work published in the field to date.

University of British Columbia ROBERT H. JACKSON

From Reform to Revolution: The Demise of Communism in China and the Soviet Union. By Minxin Pei. Cambridge: Harvard University Press, 1994. 253p. $39.95.

The central question posed in this book is why initially limited reforms in China and the Soviet Union turned into revolutions that fundamentally undermined the communist regimes. As such, the book takes yet another stab at understanding what the author refers to as the "Tocqueville paradox," that is, the tendency for authoritarian regimes to be most vulnerable precisely when trying to reform themselves. The author's explanation of this phenomenon is based on the idea that reform becomes revolution when the balance of power shifts from state to society. This "societal revolution," as Pei calls it, is shaped by the character of the initial opening of the old regime, the activation of social groups, the formation of takeover coalitions, favorable external factors, and the institutional decay of the old regime.

While societal revolution is a common denominator in both the recent Chinese and Russian "revolutions," Pei notes that the route of transition is not the same in each case. China has taken the "evolutionary authoritarian route" in which marketization precedes democratization while USSR/Russia took the "revolutionary double-breakthrough route," in which democratization precedes marketization. Pei also suggests a third possibility, the "simultaneous single-breakthrough route" in which democratization and marketization occur together. His most important point in this respect is that both the specific route taken and the success of the transition attempted is largely a function of preexisting historical-structural factors. As such, Pei's analysis provides counterpoint to the "elite choice" approach, which finds emphasis in much of the literature on contemporary regime change.

The strongest aspect of the book can be found in the application of the analytical framework to an understanding of the process and outcome of economic change in Russia and China (chaps. 3 and 4). Pei does an excellent job of contrasting China's capitalist revolution with the economic "nonrevolution" of the USSR in the *perestroika* era. At the same time, he poses—and effectively answers—the very interesting question of why *perestroika* did not spark a societal economic revolution in the USSR in the same way that *glasnost* sparked a political revolution.

Compared to the high standard of analysis set in his examination of economic change, Pei's discussion of liberalization of the mass media in China and the USSR (chaps. 5 and 6) is not quite as effective. That is not to say that the discussion is without merit. On the contrary, Pei provides a very useful analysis of the liberalization of the media in each case with an interesting emphasis on the role of political and market forces in either stimulating or obstructing that process. What is missing here, however, is the rigorous and parallel application of the analytical framework in the same clear and effective way it had previously been applied to the issue of the economy.

In the brief conclusion to the book, Pei asks both which route of transition (the Chinese or Soviet) represents the less costly exit from communism and which is most likely to lead to the long-term establishment of a democratic political system based on a market economy. Though appropriately agnostic where required, Pei cannot entirely conceal an apparent preference for the Chinese "evolutionary" approach. But the issue here is less Pei's answer than the questions themselves. If it is the case, as Pei argues, that the transition route is largely determined by initial conditions and is not merely a matter of elite choice (pp. 3–6), is there any point in raising questions about the relative costs or viability of each route? Given Pei's previously expressed analytical determinism, one might argue that the costs of the Russian transition are less appropriately compared to the costs of China's transition than to the cost of resisting transition in Russia altogether. Moreover, one wonders at this point what happened to the third route, simultaneous single-breakthrough. Traveled, according to Pei, in neither Russia nor China nor in any of the other postcommunist transitions, one has to wonder if it should practically—or even theoretically—be treated as a distinct third path.

One final criticism is also in order; Pei is too quick to dismiss the utility of comparisons of transitions from communism with other transitions from authoritarian rule. Notwithstanding his obvious point about the dual character of postcommunist transitions (i.e., economic and political), it seems to me that his own analytical framework and emphasis on "societal revolution" could easily be extended to noncommunist case studies. In a sense, Pei might be seen as too modest. He has developed a conceptual framework that allows one effectively to peer inside the Tocqueville paradox. In so doing, he makes a real contribution to the existing literature on regime change by analyzing in which circumstances reform will, in fact, lead to revolution and in which circumstances it will not. Thus, the approach has a potentially broad application, and this book should therefore be read with interest by scholars and graduate students whose countries of specialization extend well beyond China and Russia and even beyond the communist world.

Dickinson College RUSSELL BOVA

Dismantling the Welfare State? Reagan, Thatcher, and the Politics of Retrenchment. By Paul Pierson. New York: Cambridge University Press, 1994. 213p. $49.95.

A central message of Paul Pierson's important new book is that theories of welfare expansion do not take us very far in understanding the politics of welfare retrenchment. Indeed, the central contribution of the book is to offer a theory of retrenchment that, while building on recent work, emphasizes the distinctive character of the interests, strategies, and constraints involved in efforts to dismantle the welfare state.

The substantive focus of the book is Reagan and Thatcher's efforts to significantly shrink the welfare state. Pierson provides informative, finely grained analyses of the politics of programmatic retrenchment in old-age pensions, housing policies, and income support programs (and less detailed studies of health, sickness and disability programs). He also offers a general treatment of the fortunes of "systematic retrenchment"—efforts to alter public opinion, revenue-raising, group power, and institutions. These factors condition future programmatic retrenchment. Pierson's main substantive conclusions are as follows. Thatcher was able to significantly cut and restructure public pensions, council housing, and unemployment compensation. Thatcher had less success in other program areas and, with the exception of weakening labor, in efforts at systematic retrenchment. The programmatic impact of 1980s U.S. deficit finance is the best example of successful systematic retrenchment. The Reagan administration enjoyed more limited success than Thatcher in cutting pensions, housing programs, and unemployment compensation and suffered failures in efforts to go beyond modest benefit and structural changes in means-tested programs. Overall, radical retrenchment did not occur in either nation.

Pierson's main concern is to explain the fortunes of retrenchment efforts across programs within each country and the differential outcomes. Pierson argues that class power resources and political institutions, two sets of factors emphasized in recent work on welfare expansion, have limited utility in explaining retrenchment success and failure. For instance, the relative power of unions in the United States and Britain can not explain Thatcher's greater success in cutting public pensions. (Pierson does note, correctly I believe, that such conclusions would differ had the case studies included comparisons of Sweden and the United States.) Similarly, constitutional arrangements (e.g., federal versus unitary institutions), administrative capacities, and the policy initiatives of autonomous bureaucrats can not explain retrenchment outcomes very well either.

As an alternative, Pierson argues that, unlike welfare expansion, the politics of retrenchment involve the political costly enterprise of taking away concentrated benefits from specific groups in return for diffuse and uncertain benefits. As a result, policy makers must employ strategies that minimize the political costs associated with the mobilization of constituency groups. To that end, the Reagan and Thatcher administrations employed strategies of obfuscation (e.g., blame avoidance, low visibility policies), division of constituencies, and compensation (i.e., side payments). However, the efficacy of these strategies is highly dependent on the policy feedback of extant program structures. Past policy arrangements in the developed welfare states differentially distribute resources and incentives for group mobilization, "lock-in" networks of social and economic interdependencies in varying degrees, and unequally distribute information to constituencies on interests, government performance, and potential allies. Thus, the variability of past program structures effectively determines the efficacy of retrenchment strategies. Overall, the weight of these policy legacies significantly contributes to the resiliency of the welfare state.

Generally, this theoretical framework works well in answering Pierson's central questions. For example, for pension policy, the fragmented, uneven character of British pensions along with peculiar indexation and contracting-out provisions allowed Thatcher to successfully pursue retrenchment strategies of decremental, low visibility change (i.e., obfuscation) and division. The mature, earnings-related, unified social security system in the United States with its locked-in, long-term financial commitments created large, mobilized constituencies and high-levels of public support; retrenchment strategies were more constrained and largely confined to the "trust-fund crisis" of the early 1980s. Overall, Pierson's theoretical framework provides much insight and understanding of the social welfare policy records of Reagan and Thatcher.

The many merits of the book notwithstanding, one might offer some constructive criticisms. First, while the theory certainly has some broad applicability beyond the United States and Britain, one might consider its generalizability. With its emphasis on the differential program characteristics within variegated structures, and the dynamic interplay of policy makers and a multiplicity of interests, one might wonder how well the theory will explain retrenchment politics in more comprehensive, universal welfare states and in social democratic corporatist or statist systems.

In addition, the study might have benefitted from more systematic theoretical treatment of two sets of forces. First, while Pierson is quite sensitive to the foundational roles of post-1973 macroeconomic performances and fiscal stress in welfare retrenchment politics, there is little in the way of systematic treatment of transformations in employment structures and production processes as well as increasing globalization of markets and competitive pressures. These influence the ideology and content of retrenchment efforts, condition the interests of labor and business and may affect the politics of programmatic initiatives. Second, while it is certainly reasonable to delimit the focus to core social welfare policies, the opportunities and constraints pertaining to welfare policy retrenchment are most certainly conditioned in complex yet systematic ways by shifts in macroeconomic orthodoxy, policies and taxation. While Pierson addresses these sets of forces in brief concluding sections, earlier, more systemic theoretical attention would have strengthened the study.

Despite these observations, this important book can be highly recommended to students and scholars. It is particularly appropriate for advanced undergraduate and graduate courses stressing national institutions, public policy, or political economy. It is essential reading for welfare state researchers and other scholars interested in the politics of social policy in the contemporary era.

Marquette University DUANE SWANK

Incomparable Worth: Pay Equity Meets the Market. By Steven E. Rhoads. New York: Cambridge University Press, 1993. 334p. $24.95.

Comparable Worth: Is It a Worthy Policy? By Elaine Sorenson. Princeton, NJ: Princeton University Press, 1994. 166p. $29.95.

Comparable worth policies aiming to correct the allegedly systematic underpayment of female-dominated jobs have constituted one of the most controversial egalitarian reform projects in liberal capitalist societies over the last two decades. Steven Rhoads and Elaine Sorenson both embrace the discourse of economics to analyze actual experiences with comparable worth (or pay equity, as it is also called) implementation in particular venues. The two authors begin from quite different conceptual positions, however, and reach quite opposite conclusions about the subject.

Rhoads's study begins with a relatively balanced overview of arguments for and against pay equity. He then features three case studies—the state of Minnesota (chaps. 4–5), the United Kingdom (chap. 6), and Australia (chap. 7)—to examine the process and outcomes of comparable worth implementation. Rhoads's analysis builds largely on statistical studies by other scholars, along with an array of opinions and anecdotes gathered from his own interviews with persons involved in, or affected by, reform.

In Rhoads's view, sex-based wage disparities reflect simple forces of supply and demand. His argument turns on the assumption that women tend to "prefer" jobs with flexible schedules and shorter hours, thus producing an excessive supply of workers and driving down wages for specific female-intensive occupations. Consequently, Rhoads argues, comparable worth proponents' goal of remedying gender-based wage discrimination is grounded in a misunderstanding about the actual character of women's labor market behavior. Even worse, in his view, comparable worth imposes a "government-determined" process that insulates wages from market forces at the high cost of diminished economic efficiency. Artificial wage policies sustain incentives for people (especially women) to prepare for already-crowded occupations while depressing overall employment levels. Rhoads uses his case studies to show that many women, thus, are not helped—and some even lose—by reform, while productive persons (mostly males) must pay the costs for change. Moreover, the comparable worth implementation process itself, which relies heavily on dubious job evaluation techniques, is highly subjective, inherently political, and unduly disruptive to workplace relations. In sum, comparable worth is a "mix of sham science and employee participation" (p. 122) that would be disastrous for our economy if attempted on a large scale.

Rhoads's book highlights some important aspects of comparable worth. Above all, he illuminates the complex, occasionally surprising impacts of wage reform for certain groups of people in the workplace. Rhoads's study also provides voice to critics directly affected by comparable worth, including persons who supposedly should benefit from the policy. And even many proponents of pay equity will agree with some of his criticisms of specific reform policies in several of the case studies.

But Rhoads's overall analysis is unsatisfying. First, the evidence for his most basic arguments is thin. He cites studies conducted by others without scrutinizing their methodologies or assessing well-known criticisms of such work. Moreover, the many pages of random quotes and anecdotes—most from expected critics such as managers, personnel experts, male workers, and free-market economists—on a wide variety of often idiosyncratic issues simply do not add up to a compelling case against comparable worth.

Even more important, Rhoads's analytical framework is unconvincing. Written in a journalistic style of exposé, it relies heavily on repeated revelations of the commonplace (e.g., that women want more money, that comparable worth reform processes are political, that conflicts exist among workers). Furthermore, his key concepts are underdeveloped and lacking in fine distinctions. In particular, the idea of the "market" is more a shrine than a refined, complex analytical tool in this study. Rhoads's discussion repeatedly draws a clear line between, on the one hand, "objective," efficient market forces of supply and demand that reward productive workers and, on the other hand, highly "subjective," arbitrary political processes dominated by self-interested women and "left-of-center" advocates. The fact that what we call market relations themselves are complex and shaped, in large part, by law, government action, and political forces of all types seems to escape him.

A related problem is that Rhoads often fails to disentangle the complex strands of discrete factors at work in his case studies. This tendency is most evident in his treatment of Australia as "a key test" (p. 180). The Australian example is summoned to support Rhoads's general admonition that comparable worth in the United States could start us on a slippery slope toward a highly centralized, mandatory wage policy. But the argument does not hold up. Rhoads's conflation of comparable worth policies with other features of Australia's rigid industrial relations system undercuts both his causal argument and the relevance of comparisons with the United States, where the character of wage equity politics and workplace relations has been very different. The author himself admits in several places that the evidence is "murky" and the case study, problematic; but he makes much of it anyway. Overall, Rhoads's argument is likely to win favor among those who are already true believers in free-market ideals, but he will make few new converts with his study.

Rhoads repeatedly hails economists and praises their studies. Elaine Sorenson is an economist who both critically assesses studies by other economists and offers her own original econometric analysis of pertinent data. Her study begins by examining evidence regarding the historical underpayment of women in the United States. She argues that most earlier studies fall short by failing to control for industries (most equity reform focuses on disparities within, rather than among, firms) and by focusing on occupations, rather than on individuals. Sorenson's model, by contrast, incorporates these features in analyzing data from two sources: the 1984 Panel Study of Income Dynamics and the May/June 1983 Current Population Survey. Her finding is that 20–23% of the national pay gap is explained by sex composition unrelated to productivity. This is slightly lower than some earlier estimates by advocates, but it establishes for her a verifiable target of bias that should be remedied by comparable worth reforms.

In chapter 3, Sorenson provides a brief but compelling assessment of leading explanations for the wage gap. She contends that neoclassical economists' "occupational choice" models (favored by Rhoads) are less convincing than "discrimination" theories. Here, she rejects legalistic conceptions of discrimination (which turn on employer intent) for an economic conception of discrimination that exists whenever "two groups of workers are paid differently even though they are equally productive" (p. 43). One of Sorenson's most original and interesting arguments is that "institutional" theories of discrimination best explain dynamics in the public sector, while "crowding" theories best make sense of private-sector discrimination.

Sorenson analyzes, in chapter 4, the process of comparable worth implementation. Like Rhoads, she acknowledges that comparable worth implementation (esp. the job evaluation process) can be subjective, arbitrary, and unfocused. But she offers specific solutions to help minimize these problems. Her advice stresses the need to target only underpaid women's jobs, thus deemphasizing job evaluation (pay for points) and reducing reform costs. Chapter 5 then analyzes the effects of comparable worth reform in Minnesota, which in her view is the best model of the 20 or so states where it has been tried. Sorenson's statistical analysis paints a rather different picture of the Minnesota experience than that offered by Rhoads. She found that the ratio of women's to men's wages rose from 72% to 80.5% over four years (a 12% relative increase), while women's relative unemployment rose by only an insignificant amount (1.7%). In sum, comparable worth was "successful" in that it provided "substantial benefits to women and produced little, if any, negative employment effects" (p. 114). All in all, Sorenson concludes that there is a "serious problem" of inequity in the U.S. labor market, for which comparable worth reforms provide a limited and imperfect but still necessary reform response.

I find Sorenson's the stronger analysis of the two. This may be because, admittedly, I am a cautious supporter of pay-equity principles and skeptical about strict fidelity to neoclassical economic theory. But Sorenson's book surely offers more sophisticated social science. Her study is theoretically focused, parsimonious, and rigorous. Moreover, it offers well-conceptualized econometric analysis of relevant data. She faces the dilemmas of comparable worth head-on, offers critiques of past reform efforts, and suggests policy proposals that can address past problems.

This is not to say that there are no weaknesses in Sorenson's analysis. While her examination of legal issues is useful, she underplays the potential significance of the 1991 Civil Rights Act for renewed litigation challenging systemic discrimination and shows little awareness about the complex role of legal tactics in reform politics. Indeed, the biggest limitation of her policy approach is in dealing generally with the politics of reform. Her analysis of both the key actors and contextual factors that shape equity conflicts is narrowly elite-focused and technocratic. The "messiness" and conflict that Rhoads identifies in comparable worth implementation is mostly ignored by Sorenson. Similarly, Sorenson's heavy reliance on aggregate statistics limits her ability both fully to engage critics like Rhoads and to account for the varying roles, values, and perspectives of the people (esp. working women) most involved in, and affected by, reform. In particular, her study isolates wage reform from concerns about overall workplace power relations, union organizational strength, and meaningful participation that often are as important to women (and men) workers as are wages.

These books are valuable for their contrasting approaches to economic analysis of comparable worth. But each, in its own way, slights politics. Rhoads's argument condemns the very existence of politics in workplace matters and praises, instead, worker deference to employer authority and elusive market forces. Sorenson's treatment of comparable worth policy primarily as a technical matter may prove helpful to equity proponents, but she gives only limited attention to the politics of workplace reform itself. Readers primarily interested in the intricate play of power and politics in battles over comparable worth thus may find other books on the subject more helpful.

University of Washington MICHAEL W. MCCANN

Comparative Constitutional Engineering: An Inquiry Into Structures, Incentives and Outcomes. By Giovanni Sartori. New York: New York University Press, 1994. 219p. $40.00.

In the midst of rapid and profound transitions toward democracy in a number of political systems, many students of comparative politics have come forward to offer prescriptive remedies for constitutional deficiencies. Giovanni Sartori's essay is one of the latest in this new genre of works. The more advanced student of comparative constitutional systems is likely to have some mixed feelings about this book. The basic argument of Sartori is that a constitution is like an engine, understood as "mechanisms that must 'work' and that must have an output of sorts . . ." (p. ix). His principal concern is that theses engines (parliamentary or presidential; proportional or majoritarian) will fail if they do not effectively combine the forces of punishment and reward. The two core missions of the engine are to provide choice and conflict management. Effective and efficient choice for the public is accommodated through the electoral rules codified in the constitution. The management of a process whereby conflict resolution can be achieved without resort to the collapse of the democratic edifice is achieved primarily through the constitutional balance between the executive and the legislature. Modern anti-politics make these objectives increasingly difficult to achieve. Thus, the need for some basic constitutional engineering.

With an eye to democratic effectiveness (i.e., persistence of democracy) and efficiency (i.e., stable leadership), he reasons that modern constitutional democracies must move toward a combination of presidential and parliamentary leadership (i.e., alternating presidentialism). This would consist of (1) a fixed term of office where a parliament was elected only once during the period, (2) a president who would take over when and if the parliament failed during the period, and (3) a president and a parliament that would be elected simultaneously, with the president elected by an absolute majority method, and a further proviso that the president and the parliament be elected through a double ballot method. The alternating presidential system would avoid frequent elections which "overheat politics and bring about an excess of irresponsible electoral politics; and [where] there is no assurance that new elections will produce different parliaments and thus provide a solution for the problems that prompted that early call" (p. 158). Furthermore, Sartori argues that alternating presidentialism will discourage the cyclical "budget-busting spending spree" that he asserts weakens democracy prior to elections. Parliament, he suggests, will be too weak to bother with such pre-election tactics, while the President can parlay his/her constitutional position into a "countervailing player . . . a 'corrector' of parliamentary feebleness and largesse" (p. 158). Thus, effectiveness and efficiency will be better served.

This is a straightforward and direct indictment of most modern democracies, replete with a clear and reasonably well articulated prescription for escaping the flaws of too much democracy. It is also a facile argument and remarkably anti-democratic by implication. First, he has little tolerance apparently for untidy politics. Democracy is, unfortunately, untidy. It does not easily conform to efficiency, and often what appears ineffective on the surface, measured over a short time frame, is later found to be rather effective in perspective. To draw on Weimar Germany and the French Fourth Republic as the defining examples of the dangers of too much democracy misses the point. Furthermore, his analysis of the contemporary plight of politics in chapter 8 ("The Difficulty of Politics") reveals an impatience with the natural and unavoidable consequences of modern information, born of free choice and the ideal of tolerance. If some find too much information boring and tune out to politics, it seems difficult to believe that a few turns of the constitutional wrench are really going to work—especially if by work we mean a return to the old time religion where the elite ran the show behind closed doors in smoke-filled rooms.

A more fundamental problem with the book is its lack of any analysis of the reasons why constitutional systems are designed the way they are. Obviously, these are often quite idiosyncratic. Yet, to suggest that contemporary political scientists can engineer better democracy without considering the foundations of the original design is equivalent to a physician performing a blood transfusion without knowing the blood type of the patient. How can anyone assume that redesigning the rules of democratic politics is easily going to alter the motivations and traditions which gave rise to the apparent problem in the first place? The real issue is simply this: does culture dictate the constitutional balance and equilibrium, or can the constitutional design itself somehow alter the patterns of behavior and norms of society? This is more than an academic question for it is fundamental to any exercise purporting to engineer a normatively superior democracy. Sartori simply avoids this issue, and aims straight for the prescriptive cure, not sure nor apparently interested in, it seems, the potential unintended consequences that might arise from the subsequent mix of structure and culture.

Ultimately, students of comparative constitutions are likely to walk away from this essay convinced that engineering should be left to engineers. Political science can best serve the reform process by having answers to basic questions and the courage to admit that real world politics often deviate from textbook theories.

Texas A&M University JOHN D. ROBERTSON

Modern Latin American Revolutions. By Eric Selbin. Boulder: Westview, 1993. 244p. $49.50 cloth, $16.95 paper.

Eric Selbin has written an important book. It makes the most sustained recent argument that ideas and actors, not structures and large historical forces, are the primary shapers of modern revolutions. Criticizing structural and mobilization approaches that minimize the importance of "vision" and charismatic leadership, Selbin offers a brief in favor of bringing people, their goals, and their actions "back in" to the analysis of revolutions.

Selbin introduces a useful distinction between the *institutionalization* and *consolidation* of a revolution. Institutionalization is the objective face of postrevolutionary state making: To what degree are there new courts, new military and police forces, and new executive and legislative bureaucracies that function to maintain the new regime? Consolidation is the subjective side: To what degree do people come to trust the new regime, believe in its goals, internalize its revolutionary project? Institutionalization may be strongly dependent on structures, resources, and historical conditions. But consolidation is another matter: this cannot be enacted by decree from above. Consolidation requires an inspiring vision, ideological leadership, and specific policy choices that give a sense of empowerment and win the allegiance of the population.

Selbin grants that these aspects of the revolutionary process have a dialectical relationship: strong consolidation can help undergird effective institutionalization, and vice versa. Yet in the last analysis, the two aspects are not reducible to each other and can vary independently. Selbin chooses four cases of modern Latin American revolutions to demonstrate this possibility: Nicaragua (strong institutionalization and strong consolidation), Bolivia (strong institutionalization but weak consolidation), Cuba (strong consolidation but weak institutionalization), and Grenada (weak on both counts). The choices are excellent. Not everyone may agree that all the cases conform to any definition of revolution (including Selbin's). Yet the cases are selected precisely for their high degree of variance, and they succeed in illustrating the differences Selbin discusses.

To begin with Cuba, the continued domination of Castro and his ability to direct Cuba's politics, despite great economic difficulties, shows the strength of the revolutionary project and the regime. Far less certain—whether there is any Cuban revolution beyond Castro: Where are the political parties, stable bureaucratic structures, established rules of the game, and widely accepted political goals that will make for institutional stability once Castro passes from the scene? In Selbin's view, this is precisely what Cuba lacks, despite Castro's charisma, his still-appealing vision of revolutionary defiance of capitalist ogres, and the very real successes in literacy, basic nutrition, and health care that initially won over the populace. Consolidation, in and through Castro, has been achieved—but without solid institutionalization. Bolivia represents the opposite picture. The initial victory of the MNR reset the institutional structure of Bolivian politics. Yet the failure of the MNR to win the people's trust and maintain a clear vision of the revolutionary project led to intraparty squabbles, the imposition of conservative fiscal policies, and the dissolution of the revolution into elite-led "politics as usual." In Selbin's view, the Bolivian revolution failed not because of lack of popular support nor because of any strength or outside support of the old regime but primarily because of a lack of leadership.

Nicaragua and Grenada fill out the high and low ends of this typology. In Nicaragua the ability of the Sandinist institutions to survive and function even after the electoral defeat of the FSLN demonstrates successful institutionalization. At the same time, the *inability* of the conservative forces on the right wing of the victorious UNO to reverse the FSLN's social legislation or take control of the armed forces—indeed, in any way to "roll back" the revolution—suggests a respect for the validity and accomplishments of the revolution and for popular support of its goals. Successful consolidation meant that respect and support for the revolutionaries' goals could continue even if the electorate no longer judged the FSLN the best leadership to reach those goals. Grenada, on the other hand, could neither institutionalize nor consolidate its revolution, which petered out in internal coups and popular rejection of the new regime.

This brief but scholarly book has its weaknesses. The case studies are no more than thumbnail sketches, designed to illustrate, rather than prove, the author's views. The criticisms of structural arguments are sometimes overdrawn, and the characterization of particular events or leaders in the cases is sometimes facile and superficial. This work is a cry that something is missing in the dominant paradigm, rather than an attempt to go beyond it; there are no efforts at synthesis or inquiry into the causes or outcomes of revolutions in other times or regions.

Nonetheless, there is clearly room for such a synthesis. I have argued that structural changes created state breakdowns, but ideologies determined whether these would develop into socially transforming revolutions. Selbin emphasizes the role of actors and their ideologies in shaping a revolution's character and outcomes, rather than in creating the political opportunities for such redirection. Indeed, he grants that "charismatic figures cannot by themselves move a society into revolution, and on occasion may in fact find themselves rushing frantically . . . to keep up with events" (p. 84). Nothing in Selbin's analysis precludes a view of revolutions as chiefly structurally precipitated but chiefly ideologically directed.

Despite its empirical slenderness, Selbin's conceptual contributions are significant, and this book deserves to be widely read. The distinction between institutionalization and consolidation should win wide use. If the next move in theories of revolution is greater attention to matters of ideology, individual action, and outcomes, as I believe it will be, Selbin's book is an important step.

University of California, Davis JACK A. GOLDSTONE

Winners and Losers: How Sectors Shape the Developmental Prospects of States. By D. Michael Shafer. Ithaca: Cornell University Press, 1994. 270p. $37.50 cloth, $14.95 paper.

A leading concern of comparative political economy involves relations between state and civil society and how they affect the distinctive patterns of development. Students of developing countries are interested particularly in the pathways by which societies move toward development, and the degree to which structural influences affect their progress. Central to those issues are economic sectors—the intersection of domestic development with international political economy.

In *Winners and Losers*, D. Michael Shafer argues that economic sectors shape the development prospects of nation-states and that analyzing those sectors will yield an integrated vision of development from the macro scale of the international division of labor to the micro-scale choices of sectoral actors. The author sets out to link structure, agency, and choice in a way that permits differentiation among developing country cases as they integrate into the international system. Though Shafer makes his argument with reference to cases and sectors in the developing world, he is specifically committed to an approach that does not distinguish among developing and developed countries a priori (p. 18). The four cases and sectors, as defined by Shafer, are Zambia, copper mining; Korea, light manufacturing; Sri Lanka, industrial plantation crops (tea); and Costa Rica, peasant cash crops (coffee).

This notion of sector is not simply primary, secondary, or tertiary or even the idea of branches of economic activity as rendered in economic census material (transport, communications, industry, agriculture, mining and energy, etc.). Instead, this volume treats sectors as a complex of activities that correspond to a sort of sociology of economic organization at the macrosocial scale.

The author compares the sectors and cases in terms of the degree of their capital intensity and economies of scale, which are supposed to relate to production and asset/factor flexibility. The argument is, in part, that sectors shape development possibilities because they "have an optimal, or at least typical, economic organization," which affects their ability to meet the challenges of a dynamic economic environment. His goal in the volume is to "construct a truly comparative, causal theory of the political economy of development" (p. x). This is an ambitious venture; and unfortunately, the case research does not live up to the challenge. Causality is never established: the cases are descriptive, and the argument for causality is not subjected to much of a test.

The author's argument suffers from inattention to both established bibliography and readily available similar cases. The result is a sweeping set of generalizations that undermine his more specific contribution to the literature. For example, Zambia is presented as a victim of copper, with its rigidities and dependence on international markets. But the treatment of the copper cartel is cursory, and the obvious comparison with Chile is totally ignored. Through sectoral diversification, Chile has overcome many of Zambia's weaknesses, while continuing to depend on, and wrestle with, copper as a key sector in its foreign exchange profile. The book's conclusion includes comparison of Zambia with the petrostates, which is an interesting afterthought, as oil was always considered "the great exception" to the commodity enclave trap. But even there, Shafer relies almost exclusively on one volume in a broad, demanding, and theoretically rewarding literature. This same criticism applies in different degrees to the author's treatment of the international division of labor, multinational corporations, "Dutch disease," export enclaves, and other important literatures directly related to his arguments.

Other pitfalls of wide-ranging cross-case comparisons are also apparent. Reference to the politicization of the Sri Lankan civil service in the 1950s does not even mention the importance of communal strife during that period. Tea is characterized as a "perfect plantation crop" that "cannot be produced competitively any other way" (p. 147)—ignoring the case of Kenya, which during the same period of study was the largest black tea exporter in the world, based not on plantations but on over 100,000 smallholders and a national agricultural marketing structure governed by the Kenya Tea Development Authority. Coffee, in contrast, is seen as the classic peasant cash crop in Costa Rica—a stereotype that does not travel well even within the Central American region, as the author notes in his conclusion. If the "sectoral types" laid out in this volume are truly typical, what do the deviant cases tell us? The answer lies in a clearer typology that draws on the theoretical and empirical literature available.

This volume addresses an interesting question: What is the role of sector in development paths? But the questions posed by the author demand an understanding of path dependency, an exhaustive treatment of the cases under study, and an appreciation of the limits of sectoral analysis to overall political behavior. Its contribution to comparative political economy would be enhanced by a more complex model of intersectoral relations, as well as a clearer method for choosing cases.

University of Florida STEVEN SANDERSON

INTERNATIONAL RELATIONS

The Challenge of European Integration: Internal and External Problems of Trade and Money. Edited by Berhanu Abegaz, Patricia Dillon, David H. Feldman, and Paul Whiteley. Boulder: Westview, 1994. 316p. $59.95.

This book presents a lively policy dialogue concerning the implications of Europe's recent Maastricht-related reforms. In particular, the chapters (and various comments on chapters) cover such issues as preferential trading arrangements and their relation to the GATT/WTO system, the likelihood of effective monetary integration, and the prospects for political and institutional development. The book is a hybrid, primarily made up of material generated by a symposium, and includes both articles-with-commentaries and narrower pieces on other topics. The result is a book with very broad coverage but somewhat weak integration of material. The reader should anticipate a discussion that is primarily policy-oriented rather than theoretical, particularly where politics is concerned. These criticisms aside, the text is a useful tool to bring those already well acquainted with European Union history and institutions up to speed on the burning questions of the day in Brussels.

The first section deals with European history as it relates to the implications of Maastricht. The author of the centerpiece article, Barry Eichengreen, argues that even if Europe, Asia, and the Americas move deliberately toward exclusive trading-bloc arrangements, the welfare and political implications will be small (if not negligible) and may even be positive, so long as macroeconomic policies remain open and responsible (the key failure of the 1930s). The discussants challenge this optimistic prediction, although they do not disagree with the basic analysis.

The next two sections relate specifically to economic issues of integration. A piece by Beth Yarbrough asks whether the European Union and other regional trade bodies are generally beneficial to industrialized and developing nonmembers, with the answer that they are generally compatible with global liberalism and growth, except perhaps in relation to developing nations. Discussants mildly disagree with the optimistic implications but, again, do not challenge the basic analysis. Morris Goldstein and Jacob Frenkel consider the potential for, and desirability of, monetary unity in Europe, arguing that in theory, a disciplined single-currency system would bring far more benefits than costs, assuming countries are willing to cede control over day-to-day monetary policy. The discussant asks whether the authors have carefully considered the political implications of monetary union—which they have not.

In general, the analysis of economic policy in the text assumes a successful Maastricht, that is, an ever-deepening European Union that will culminate in a fully integrated system in the medium term. There is little consideration given to the possibility that either internal or external tensions will lead to serious cleavages within the union. When such possibilities are confronted by the authors, they generally assume that the sheer force of economic rationality will resolve them or keep them in abeyance. The benefits of integration so clearly outweigh the costs, at the aggregate level, that even nonmembers will welcome greater unity in Europe. The only wrinkle the authors seem to acknowledge is the potential net loss for developing nations and the former Soviet bloc. This rosy scenario seems to reflect more the authors' normative bias in favor of integration, rather than any empirically obvious recent trends.

The final section on political and institutional development is the most intriguing and provocative, if more because of what is left out than what is put in. The centerpiece of the section is an interesting review of Maastricht and some of its legal and political implications by Phillippe C. Schmitter. He argues that the treaty contains the seeds of a more powerful and active European Community institution but will likely lead to a hodge-podge of overlapping and competing bodies, each with uncertain authority and offering numerous opportunities for states to opt out of their commitments. The ensuing debate among commentators centers on how likely countries will be to submit to European Union directives when they conflict with national interest. The political dilemmas (legitimacy, sovereignty, etc.) are then spelled out nicely, but the discussion lacks a consideration of the role of national and regional courts in this process. This is a serious gap, since, after all, it is largely the result of the European Court's judgments and national courts' responses to these that many conflicting national policies have been forced into regional harmony. Also, while it fails to develop any theoretical framework that builds on existing literature, the discussion makes frequent mention of the significance of existing theoretical models, thus leaving the reader somewhat disappointed.

In general, the book is a useful starting point for discussions of current European Union policy issues. To be a practical contribution to a graduate course, however, it would need to be introduced with considerable background material on European Union history and institutions, as well as theoretical literature on integration and cooperation. It would also help if the price tag were lowered with the production of a softcover edition.

Bowling Green State University KENDALL W. STILES

Game Theory and International Relations: Preferences, Information and Empirical Evidence. Edited by Pierre Allan and Christian Schmidt. Aldershot, UK: Elgar, 1994. 213p. $74.95.

The award of the 1994 Nobel Memorial Prize in Economic Science to three game theorists signals a coming of age of the theory, born 50 years ago with the publication of Neumann and Morgenstern's *Theory of Games and Economic Behavior* (1944). Game theory's development, however, has been decidedly uneven, ranging from esoteric mathematical work to down-to-earth practical advice.

The present book is in that middle category, neither highly mathematical (mathematical derivations are confined to appendices) nor directly applicable to the selection of optimal strategies in, say, a real-world crisis. Rather, the editors have put together articles that use noncooperative game theory to explicate key strategic features of certain processes (e.g., negotiations) and to illustrate these with case studies of actual negotiations on the Polish debt, European integration, and migration issues. Other articles are concerned with the raw material that goes into a game (e.g., preferences) and what it is reasonable to assume about their formation, aggregation, and representation by utility functions.

The book begins with an article by Vinod Aggarwal and Pierre Allan, who define "constrained preference orders" for two players based on their overall power positions, their issue-specific powers, and the strengths of their domestic coalitions. Although such capabilities may well constrain preferences, making them the *sole* determinant of preferences is, in my view, misguided. (Why cannot a weak player prefer an outcome that only a strong player can induce?) One should, I believe, start from preferences, which are fundamental to the idea of players' making rational choices. Given these, one can then define a player's power or strength in terms of its ability to achieve an outcome better than what it could normally attain in a game (e.g., by selection of its dominant strategy).

Aggarwal and Allan's assumption that "actors proceed with moves and countermoves, up to and until they end up in a Nash equilibrium where they will remain" (p. 17) is not only vague but also ignores the fact that reasonable sequential rules of play can make departures from Nash equilibria rational. This may have happened over the five periods of the Polish debt negotiations identified by the authors, which they explain by saying that the game kept changing—giving rise to different Nash equilibria—without explaining why these changes occurred (i.e., they offer no dynamic theory to explain endogenous changes in preferences or power relationships).

Another problem that bedevils applications of game theory to international relations is that nation-states are not usually single players but amalgams, raising issues of social choice that Lars-Erik Cederman addresses in his article. His resolution of these in terms of a voting model that results in five well-known symmetric games seems to me far-fetched, in part because, as Cederman concedes, the sequential playing out of conflicting interests to produce a "national interest" may be inconsistent with choosing Nash equilibria in simultaneous-play games.

Michael Nicholson goes back to basics in proposing that a player's utility in a two-person game be broken down into two components: a *primary* one, based on the player's own payoff from an outcome, and a *contextual* one based on the other player's payoff. Whether the contextual payoff reflects a player's benevolence, malice, or superiority toward the other player, Nicholson shows that as the weight placed on the contextual component changes, there may be radical transformations in the game. He skillfully interprets these results, which involve subtle issues at both a conceptual level (e.g., involving interpersonal comparison of utilities) and at a substantive level (e.g., concerning the relative-gains debate in international relations).

Christian Schmidt starts his analysis simply: outcomes in games are either acceptable or unacceptable to players. He next assumes that the players have second-level preferences (reflecting benevolent or malevolent attitudes toward how the second player, as well as the first, fares at each outcome), which leads to several different ordinal games, about which there may be incomplete information. This is a parsimonious and insightful framework, but I found Schmidt's rather casual application of it to different international crises less than convincing, in part because the dynamic *tâtonnement* process he uses to explain crisis behavior does not mesh with his reliance on Nash equilibria in simultaneous-play 2 × 2 games.

Bargaining over European integration policies, Gerald Schneider contends, can be conceptualized as a principal-agent problem between constituents (principals) attempting to control their governments (agents). The game-tree models he offers are plausible but not testable in a strong sense (especially when information is incomplete and the equilibria depend on numerical thresholds), making the main value of these models heuristic.

This is also true of Cédric Dupont's alternating-offers bargaining model. Like Schneider, Dupont goes to considerable length to show how his model of two-level games illuminates—even if it is not tested by—real-life cases of negotiation.

Urs Luterbacher and Jean-Paul Theler offer only cursory evidence in support of their migration model, in which the two players are countries, one from the North and one from the South. They find, unsurprisingly, that the players end up in a prisoner's dilemma, whereby each restricts migration. But they also indicate, via a dynamic model, how the players might extract themselves from this dilemma if they take a longer-term perspective.

The serious attention that most of the contributors devote to modeling real-world conflicts and bargaining processes in international relations is commendable. Although there is confusion in some articles about how to treat power, define equilibria, and model dynamics, the more general problem that formal modelers of international relations face is what to borrow, what to jettison, and what to reformulate of mathematical game theory.

This problem is aggravated by the fact that the noncooperative part of the theory (especially as developed by economists) has become increasingly baroque and not well suited for modeling purposes. While it is not clear what emendations in this theory will prove useful in capturing important strategic aspects of international conflict and cooperation, *Game Theory and International Relations*, by providing a menu of interesting possibilities, should stimulate discussion of this matter.

New York University STEVEN J. BRAMS

The Future of the Pacific Rim: Scenarios for Regional Cooperation. Edited by Barbara K. Bundy, Stephen D. Burns, and Kimberly V. Weichel. Westport, CT: Praeger, 1995. 263p. $59.95 cloth, $18.95 paper.

After the Cold War, matters of economic rivalry and security cooperation are receiving more scholarly and policy attention. Moreover, the formation of the European Union and the conclusion of the North American Free Trade Agreement have heightened concerns about the creation of an East Asian trade bloc.

This book joins several other recent publications on Pan-Pacific regionalism. Its intended focus is "the future of regional cooperation in the widening circle of the Pacific Rim" (p. 3). While some of its chapters examine this cooperation pertaining to institutional development, intergovernmental consultation, and economic transactions, others address security issues about the prospects for, and constraints on, military détente and mutual political confidence.

The contributors come from different countries around the Pacific Rim. Over half are practitioners from the world of business or government. Several academicians with strong scholarly reputations and policy advisory roles are also represented. Despite this diversity among the contributors, there appears to be a general agreement that the Pacific Rim countries should practice "open regionalism." This concept suggests that while promoting cooperation among themselves, these countries should maintain—and even increase—their ties with others outside the region.

Several chapters present cogent analyses related to this argument. M. Hadi Soesastro offers a useful historical account of Pan-Pacific regionalism, and Nobuo Matsunaga gives the Japanese view on this regionalism. While the chapters by Huang Fan-zhang, Kim Kihwan, and Saburo Okita discuss various economic considerations and cleavages, those by Robert Scalapino, Harry Harding, and Donald Emmerson examine the political motivations and military concerns of important regional actors. These authors demonstrate an interest in policy-relevant analyses, although many other contributors' styles tend to be more descriptive and hortatory.

Indeed, many chapters are quite brief, being rather unconcerned about matters of theoretical formulation, concept development, methodological rigor, or empirical evidence. Some chapters have few or no scholarly references. Theories of regimes, integration, complex interdependence, comparative advantage, and trade propensity receive scant attention even though they address the central concern of this volume. Moreover, there is little attempt to compile or analyze systematic data that can help to track the evolution of regionalism and empirically to demarcate its fluctuating boundaries. It would have been useful, for example, to develop quantitative indicators of transaction flows (e.g., trade, investment, aid, mail, tourism) in order to measure and monitor the extent of, and changes in, regionalism. Likewise, it would have been worthwhile to investigate whether the trade-creating effects of regionalism outweigh its trade-diverting effects. After all, this evidence is crucial for assessing the idea of "open regionalism"—that, somehow, intraregional ties need not come at the expense of extraregional ties. In the absence of these analyses, the volume is unable to answer important factual questions such as these.

There is, moreover, the question of explanation; that is, which forces are important in motivating or limiting regionalism? For instance, the volume tends to overlook the interaction between domestic processes and regional developments and the interaction among the political, economic, cultural, and security aspects of these developments. To what extent do domestic processes of industrial maturation, democratization, and *embourgeoisement* affect regional cooperation; and how do regional developments, in turn, influence these domestic processes? Similarly, how is regional cooperation in one sphere (e.g., arms control) likely to spill over to, or spill back from, collaboration in another sphere (e.g., commercial joint ventures)? Is regionalism in the Pacific Rim driven primarily by market forces, and do intergovernmental institutionalization and mass civic culture lag behind business collaboration? How and why are the Pacific Rim's experiences in these respects similar to, or different from, those of West Europe and North America? There is much from the theoretical and empirical literature in business, economics, history, geography, and political science that can be usefully applied to answer these queries.

Finally, given the title of the book, the reader is naturally interested in the durability or fragility of the ongoing relations among the Pacific Rim countries. How sensitive are these relations to external shocks? Are they vulnerable to reversal? Who—what countries, sectors, firms, or groups—stands to gain or lose from a continuation of, or departure from, current trends? Which key variables are likely to influence these trends? Are they within the control of governments and/or businesses? The volume could benefit from more focused attention to these perennial issues for policy analysis, as well as to those emphasizing theoretical articulation and data scrutiny mentioned earlier.

University of Colorado STEVE CHAN

We All Lost the Cold War. By Richard Ned Lebow and Janice Gross Stein. Princeton: Princeton University Press, 1993. 542p. $35.00 cloth.

Despite its title, this book is not a study of the origins or collapse of the Cold War. The authors' main claim is that security strategies of deterrence and "compellence" lead to international crises and that attempts to deter or to compel during crises exacerbate tensions and prolonged the Cold War in the 1970s. In parts 1 and 2, they study the 1962 Cuban Missile Crisis and the 1973 October War using interviews, some declassified American documents, the proceedings of several conferences, and a large body of secondary literature. In part 3, they use these two case studies to draw conclusions about crisis resolution, crisis management, and the effects of nuclear weapons and threat.

For the most part, the evidence and arguments they rely on have been available for some time. Their arguments on the domestic sources of Khrushchev and Brezhnev's policies will be quite familiar to students of Soviet foreign policy. Their detailed description of the Cuban Missile Crisis brings together the multiple pieces of information that have become available in recent years and will not surprise students of crisis behavior who have been following the literature. Their conclusion that Khrushchev deployed the missiles in Cuba in a quick-fix attempt to even the strategic balance when the failure of his missile-rattling strategy became apparent does not add to Raymond Garthoff's careful study (*Reflections on the Cuban Missile Crisis*, 1989). For the 1973 crisis, the authors have unearthed some important new information, especially a fascinating account of the reasoning behind Brezhnev's letter of 24 October proposing joint U.S.–Soviet intervention to prevent destruction of the Egyptian Third Army, which was encircled by Israeli forces and threatening unilateral intervention if necessary. It has long been debated whether the Soviets sought a pretext for intervention, genuinely preferred joint action, or sought to compel the United States to prevent further Israeli military action. Their interviews suggest that Brezhnev was serious about joint intervention but bluffing in the threat to act unilaterally (pp. 234–38).

Although the authors present detailed case studies of two important crises, this will be a very disappointing book for serious scholars. It is very difficult—indeed, it is virtually impossible—to evaluate their causal claims because the authors do not develop a theory of the causes of the Cold War, the relationship of American containment policy to superpower crises, the relationship of deterrent and compellent strategies in crises to escalation, and the failure of détente in the 1970s. Without such a theoretical framework, one cannot evaluate whether their evidence would be consistent with an alternative explanation or whether there is any consistent relationship at all.

For example, they claim that Soviet "compellence" and the American resort to a nuclear threat in the 1973 prolonged the Cold War. At the same time, they argue that mutual American and Soviet concessions in the 1962 crisis resulted in an amicable settlement that enabled the two sides to learn the correct lessons of history. There are at least two reasons why we should question these conclusions. First, although the settlement in 1962 may have been amicable, it certainly did not end the Cold War. In fact, it was followed by a period of substantial development and deployment of Soviet nuclear weapons, resulting ultimately in strategic parity between the superpowers. Far from teaching that efforts to even the strategic balance were misguided or that deterrence is irrelevant, the Cuban Missile Crisis seems to have confirmed the importance of nuclear weapons and deterrence in the eyes of the Soviet elite. Second, to sustain their argument that "compellence" prolonged the Cold War in 1973, the authors would have to demonstrate that an amicable settlement would have led to the end of the Cold War. As I have already suggested, their 1962 case undermines that claim. Furthermore, the argument contradicts their own analysis of the origins of the 1973 crisis, which focuses on the fundamentally competitive political objectives of the two superpowers (pp. 180–81). Insofar as the two countries sought to advance their contradictory political interests under cover of détente, the 1973 crisis was just a symptom, not a cause, of the continuing Cold War. And insofar as their political objectives were incompatible, the Cold War could be ended only when the goals of one or both countries changed fundamentally.

The failure to develop their own theory could have been rectified had the authors clearly established a standard theory (or set of theories) from the literature to test their cases against; but they fail to do this, as well. They refer at times to the "conventional wisdom" or "accepted faith" that deterrence and "compellence" prevent conflicts and that measures to enhance credibility will reinforce deterrence, but this is really a straw man. Theories of deterrence posit that when the costs of a policy or action exceed likely benefits, states will be deterred. The theory does not hold that deterrence will always work or even that it will usually work. The most recent and sophisticated work on credibility and reputation in deterrence (e.g., the work of Robert Powell and James Fearon) shows not that credibility is easily or costlessly manipulated but how crucial and complicated the relationship is. At times, it seems that the authors' target is not scholarly theory at all but the lessons of history as advanced by political figures. For example, in refuting the "conventional wisdom" that the American nuclear alert forced the Soviet leadership to back down in 1973, they cite not a single scholarly source but rather Henry Kissinger (pp. 261, 285). Although Kissinger's memoirs are a valuable source of information, I do not know of any serious study of crisis behavior that takes them as unvarnished truth or as the final word on the effectiveness of his own foreign policies.

Furthermore, in explaining the resolution of the 1973 crisis, the authors provide evidence that "compellence" can be quite effective. They argue that the United States successfully compelled Sadat to withdraw his request for Soviet assistance and Israel to permit resupply of the Egyptian Third Army (pp. 271–72, 278). The authors, because they fail to develop a clear theory of crisis behavior, cannot address the most interesting question, namely, when "compellence" succeeds, when it fails, and when it makes things worse. Such an effort could have been a significant contribution to the study of crisis resolution.

Even without a systematic theoretical framework, the authors' research would be valuable if the comparison of the 1962 and 1973 crises illuminated an important theoretical insight about foreign policy or crisis behavior, as they claim in their chapter, "How Crises Are Resolved." To draw general conclusions from a limited number of case studies, one must be very careful in case selection. Comparative case studies can be used to test theories and support causal claims. To be useful for inference, however, cases must be carefully and deliberately chosen by focusing on variation in the causal variables, not on important or striking outcomes. The authors themselves justify the choice of these cases in terms of their importance, severity, and high level of nuclear threat, which severely limits the general theoretical value of their research. Suppose that Lebow and Stein are correct—that a hostile and militarized American policy of general deterrence drove Khrushchev to deploy missiles in Cuba. Does that mean that hostile and militarized deterrence policies cause international crises? Obviously not: maybe the Cuban missile crisis was unusual. The 1962 and 1973 crises are important, and it makes a lot of sense to describe each in its great complexity. But the authors cannot have it both ways: they cannot choose to study these crises because they are distinctive and then develop general conclusions on the relationship between the Cold War, deterrence, "compellence," and crises.

The effect of selection bias undermines one of the points the authors hope to make. They argue that these two cases show that strong threats lead to serious crises. But what if the relationship works in exactly the opposite direction: What if serious crises (i.e., those that threaten fundamental security interests) led to the issuance of strong threats? We would certainly want to know in which direction the causal arrow ran; but there is no way to tell from this study, because the authors chose to study two serious crises with strong threats. To test whether strong threats led to serious crises, we would need to look at a variety of crises and test whether mild crises were those preceded by mild threats or accommodation. An alternative research strategy would be to look at a variety of foreign policy stances varying from strong threats to accommodative policies and test whether there was a systematic relationship between policies and severity of crises.

The authors are asking many of the right questions.

International relations scholars and specialists in Soviet and American foreign policy have failed to study systematically the relationship between security strategies and the occurrence of crises and the relationship of crisis behavior to security relations. These are very important questions on both theoretical and policy grounds. This study offers a first attempt at answering them and will hopefully launch a careful and methodologically sound research program.

Harvard University CELESTE A. WALLANDER

China's Strategic Seapower: The Politics of Force Modernization in the Nuclear Age. By John Wilson Lewis and Xue Litai. Stanford: Stanford University Press, 1994. 393p. $45.00.

Debate is intensifying over China's status as an international security actor. A growing school of thought, comprised of many analysts found in both the West and in Asia, views China as a great power intent upon exercising strategic hegemony throughout its own region and upon challenging other global powers for influence in the structuring of a New World Order. An opposing faction regards this view as short-sighted and dangerous, citing China's internal economic vulnerabilities and the questionable authority of its central political structure as evidence of a potentially weak China. The views of both sides are too often shaped by generalizations about China's strategic and military power that are unsupported by reliable unclassified data.

This book, along with its earlier companion piece, *China Builds the Bomb,* by the same authors, makes a major contribution in overcoming the information gap about China's strategic doctrines and strategic weapons programs. Lewis and Xue have utilized a wealth of previously untapped Chinese source materials to forge the most comprehensive explanation of that country's quest for development of its own strategic nuclear force (particularly strategic naval assets) that it is possible to achieve short of gaining access to government documents and archives that will probably remain inaccessible for many years.

Rarely does there appear in the social science literature a work like this one, having great value despite the absence of a central or essential argument. Lewis and Xue offer two key observations about China's strategic weapons program: (1) that it constituted the underlying basis for Chinese industrial and scientific modernization and (2) that it withstood the worst extremes of radical Chinese politics during the Cultural Revolution. Neither of these, however, allow us to make contingent generalizations about the wider Chinese political environment or add much to existing social science theory as it applies to China's recent history. Nor is it easy for the reader to struggle through a narrative where the technological components of missile parts and submarine propulsion systems are intricately detailed. Yet the authors effectively integrate such descriptions with clear and incisive assessments of the political imperatives that intermittently sustained—and threatened to jettison—China's sea-based nuclear delivery systems. They have, accordingly, served up a plot that is sufficiently dramatic and policy-relevant to earn the patience of those otherwise less inclined to ponder about the comparative advantages of linear-versus-water-drop-shaped submarine hulls!

Several important but previously underrated factors related to China's postwar nuclear policies are particularly illuminated by this text. One is that Mao Zedong supported China's nuclear weapons program and its development of strategic nuclear delivery systems far more zealously than has previously thought to be the case. It was with his apparent blessing that China moved very early to build a "small but inclusive" arsenal that would include both land- and sea-based nuclear deterrence capabilities, salvaging China's nuclear weapons research in the 1960s. Another theme illustrated by the book was how critical Zhou Enlai's intercessions were to reconciling the intense factional warfare between China's nuclear scientists and their fanatic underlings, who sought to seize control of all the weapons development programs during the Cultural Revolution. As head of the key oversight committee for managing China's nuclear weapons development, Zhou moved often and aggressively to protect his country's scientific community from the political excesses generated by Mao's sycophants.

The authors could have usefully developed these themes further by comparing the decision-making processes described in their study with previous Western efforts to assess post-1949 Chinese decision making, underscoring where their own findings support or refute earlier studies of Chinese nuclear strategy. Another problem is the authors' tendency to treat their source material as sacrosanct. The impressive collation of source materials accompanying this text would have been even more compelling with a very brief explanation of how the methodological integrity of the research was safeguarded against Chinese sources that may have been compromised by personal biases. A related problem is that the points raised in certain notes appear to have little correlation with the portion of the text cited: at intervals, the authors appeared to be overwhelmed by their own voluminous data base. Finally, important points raised early in the text are occasionally left largely unexplained until much later in the narrative or not explained at all. For example, the instigators of harassment against the Chinese nuclear scientists are assigned prominence throughout the text, but a more comprehensive explanation of how the dissenters organized themselves at various research installations and who backed them could have added much to the overall analysis.

Notwithstanding these minor discrepancies, *China's Strategic Seapower* is a landmark achievement that unquestionably enhances our understanding of China's postwar naval nuclear strategy. No other study on the topic or on the general subject comes close to matching its authoritative explanation of how that strategy evolved and the barriers it had to overcome. This work will undoubtably provide a major foundation for further analyses of how that strategic evolution has effected—and will continue to affect—China's global status and future role in international relations.

University of Queensland WILLIAM T. TOW

The Fall of Great Powers: Peace, Stability, and Legitimacy. Edited by Geir Lundestad. Oxford: Oxford University Press, 1994. 414p. $35.00.

In the summer of 1993, the Norwegian Nobel Institute held a symposium. Organized by the institute's director, historian Geir Lundestad, it brought together European and American historians but also some economists and social scientists (including four political scientists). The result—20 papers revised in the light of discussions—saw publication just barely a year later.

Prompting the symposium, according to its organizer, were, "on the academic side," the publication of Paul Kennedy's *Rise and Fall of the Great Powers* and on the political side, the collapse of the Soviet Union in 1991. The central question thus became the *fall of great powers*: the causes of fall (as illustrated by a number of twentieth-century case studies, including Austria-Hungary, the Soviet Union, and the United States) and the consequences of fall, especially for peace, stability, and legitimacy. No consensus is claimed on causes or consequences; but legitimacy aroused considerable debate. The Soviet fall is widely attributed to loss of it, and the instability attributable to falls is seen as compensated by gains in legitimacy and self-determination.

The principal shortcoming of this project (as distinct from the considerable interest of individual contributions) is conceptual imprecision, if not outright confusion. The two key terms, *great power*, and *fall* that need careful treatment in any macropolitical analysis, are left undefined, as though self-evident. The author of the keynote paper, historian Imanuel Geiss, treats *great power* and *empire* as synonymous (as do many other of the contributors) and sees it as his main task (as though facts could ever speak for themselves) to present the "vast store of material" that "history" has accumulated about "empires." Yet what precisely do we learn, for instance, by his placing the battle of Manzikert (1071, not 1076), in which Seljuk Turks inflicted a crushing defeat on Byzantium that heralded the fall of Constantinople 382 (or was it 123?) years later and "the moral defeat in the Second Vietnam War for the USA" in the same category, namely, the "one major defeat at the periphery" that generally marks the beginning of the end for most empires (p. 37). And how are we to take the editor's observation that now that the Soviet Empire is gone, the United States and China are the last two that "still control imperial structures" (p. 384).

William McNeill shows that in the past, the norm for civilized government was "laminated polyethnic empire" (p. 4). That, indeed, was the norm in the classical era of world history, and the memory still lingers. But is the United States an empire today? I believe not, because, for purposes of macropolitical analysis, it is a mistake not to distinguish between classical (regional) empires (e.g., Rome, T'ang), early modern world empires (Mongol), modern world powers (sea-oriented) exercising global leadership in the democratic lineage (Britain, United States), and global powers challenging them (Napoleonic France, Soviet Union) often acting in an imperial mode. Failure to draw such distinctions blinds the analyst to the evolutionary processes in which such large-scale political constructions are embedded.

Has the United States "fallen"? Here again, clear distinctions are indispensable. Though its (customary) term of office is as yet unexpired, the United States is now well into the fifth decade of its occupancy of the largely informal position of global leadership. Because its sway and its authority are not what they used to be (as is not unusual for political leadership past its midterm), some declare *decline*, signaling a more or less imminent departure from office; but another option, unexamined in this volume, is a second term that would take us deep into the twenty-first century.

Immanuel Wallerstein takes decline for granted and sees the next half-century—1990–2025/50—as being "short on peace, short on stability, and short on legitimacy" (p. 331). His is an uncompromisingly pessimistic prognosis, but it does stand out among the papers as the most clear-sighted about the time frames within which such a discussion must be situated. When Robert Gilpin asks, "The Cycle of Great Powers: Has it finally been broken?," he does not position his arguments explicitly in time and dismisses the question as "unrealistic": "Carr, not Kant will have the last word on this matter" (pp. 313, 326). A better answer might be that the (long) cycle of the rise and decline of world powers is a mechanism of an evolutionary process driving a transformation of world politics over extended time frames.

University of Washington GEORGE MODELSKI

Untying the Knot of War: A Bargaining Theory of International Crises. By T. Clifton Morgan. Ann Arbor: University of Michigan Press, 1994. 240p. $39.50.

This appealing book presents a collection of original models addressing whether, and how, an international crisis may turn into a war. (The title is from a phrase of Khrushchev's, in a message to Kennedy during the Cuban Missile Crisis: the harder we bargain, he warned, the tighter we pull the knot of war.) As T. Clifton Morgan sensibly argues, crisis always precedes war, so if we want to understand why wars occur we should study why some crises develop into them while others end in negotiated settlement. In *Untying the Knot of War*, he develops some creative and insightful models of crisis bargaining, illustrates them with historical cases, and draws interesting and sometimes controversial conclusions about how the outcome of a crisis depends on the characteristics of the bargainers and the issues.

The formal models of *Untying the Knot of War* are inspired, for the most part, by spatial voting models. These models represent voters' and candidates' positions in a multi-dimensional *issue space*, and assume that a voter prefers the candidate nearest his or her *ideal point*. Typically, spatial models rely on weighted Euclidean distances, under which different voters may have different sets of (not necessarily uniform) weights on the issues. Spatial models have made many important contributions to voting theory—they do a particularly good job of addressing questions about optimal candidate positioning under sincere voting, for example.

Spatial models of crisis bargaining make for simple but suggestive and useful representations of multiple-issue negotiations. For instance, Morgan demonstrates with a series of models and examples that in crisis bargaining agendas and agenda setting can be crucial. (It is hardly surprising that a methodology based on voting theory can do this, considering the central position of these questions in voting theory; yet in the crisis bargaining context these points have received too little attention.) Other questions addressed using spatial bargaining theory are the likelihood that linkage of issues will be successful (Morgan provides an explicit measure in the case of two issues), and an original and all-too-abbreviated discussion of the role of third parties in crises, as mediators and as allies. One rather surprising conclusion from the latter analysis is that not only can biased mediators be effective, in some cases they are more effective than unbiased mediators.

Morgan also links points in issue space with bargainers' utilities. The model of crisis resolution is in fact based on a set of functions $p_i(\pi)$ that give, for each bargainer i, the probability that i will accept a resolution giving it utility π, rather than go to war. This is not a rational choice formulation, and recalls Zeuthen's risk-based approach. One advantage, unfortunately not much exploited here, is that the model can produce a probability distribution over possible resolutions rather than a specific prediction. The most likely settlement is one with utilities π_i^* that together maximize the probability of no war. Somewhat inconsistently, the probability of no war is not this maximum, but the conditional probability that independently selected utility levels fall into the overlap of *bargaining ranges,* given that all these utility levels were preferred to war (i.e., conditioned on the probability functions). The effects of bargainer and issue characteristics, such as bargaining costs, resolve levels, and war success probability, on likely bargaining outcomes and war probabilities can be assessed using models and inferences about their effects on the bargainers' probability functions.

Spatial bargaining models have a great deal to offer, and will no doubt be developed further and utilized extensively in the future. Students of bargaining, like students of many other problems, will find the advantages of a simple, flexible, and visual representation too great to pass up. In this book, T. Clifton Morgan has provided a valuable summary of the objectives and the theory, as well as detailed illustrations that not only clarify points of procedure, but also show how illuminating spatial models can be. Particularly useful is the Appendix, where the model is applied to six historical crises—the Oregon Boundary Dispute, the crisis preceding the Seven Weeks' War, the Fashoda Crisis, the Agadir Crisis, the crisis preceding the Winter War, and the Cuban Missile Crisis.

In summary, *Untying the Knot of War* is a solid introduction to what is bound to become an important and fruitful approach to crisis bargaining.

Wilfrid Laurier University D. MARC KILGOUR

Transcending the State-Global Divide: A Neostructuralist Agenda in International Relations. Edited by Ronen P. Palan and Barry Gills. Boulder: Rienner, 1994. 283p. $40.00.

This 12-chapter collection will be of particular interest to younger international relations scholars educated in a neorealist or realist tradition but ready to engage with a *neostructuralism* (a postmodernization of world-system and dependency approaches) that treats society as primary and the state as secondary while restricting the claims of theory (vs. history) at the global scale.

K. P. Moseley reviews the camps and internal debates of "critical theories, more or less Marxist in inspiration, that view capitalism in historical and relativist terms," namely, those of Wallerstein, Frank, Chase-Dunn, Abdul-Malek, and Amin (p. 125). Peter S. Taylor presents historical reasons for treating states as "constituted of the world economy as the social institutions that act as a power vortex" (p. 120). André Gunder Frank argues that a model presuming rational, purposive, deliberative, powerful, effective state actors is questionable on six counts. Ronen P. Palan essays to shift the description of the state in international society from entity toward process, from power toward information, from acts toward links.

Lawrence Wilde inspects the Kautskian misprediction that Swedish "welfare capitalism" could engender a gradual and peaceful transformation to "full-fledged socialism" and indicts it for failing to anticipate the intensity with which an increasingly open world economy would impel private businesses to treat the demise of the Swedish welfare state as essential to their own survival.

Alain Lipietz believes that the "Fordist" regime of mechanization, Taylorism, intensive accumulation, mass consumption, and monopolist regulation need not be replaced by technoglobalism, open markets, and local welfare states, because there exists the "French regulatory school" alternative—a variety of local social regulation in federative states and multinational blocs; restraint of interlocal competition; and increased leisure, not increased wages (at least in Europe), as the way of distributing productivity gains.

Barry Gills contends that South Korea's export orientation can be competitively explained by (1) a state-centric approach, (2) an international political economy approach, or (3) a neostructuralist international political economy perspective but that the third approach leaves fewer loose ends.

Christopher Chase-Dunn's introduction to his own approach and project imputes greater scope to state action, contains a more explicit political goal, and is more optimistic with regard to the achievements of the "semiperiphery" than most other contributions. It contains a large number of propositions likely to be stimulative of historical and comparative research.

Sandra Halperin argues that historically, state autonomy never existed except as a threatening possibility that was successfully repressed by the class interests and elites that it menaced. The argument is empirically wide-ranging and put forward in a manner that should lead to a large, ferocious, and illuminating gladiatorial contest that will exercise the participants and delight the audience.

Philip G. Cerny asserts the imminence of a "genuinely global capitalist economy," lacking a hegemon and a regime and needing neither but embedding, instead, a new "financial orthodoxy" and transnational financial structure whose efficiency and endogenous stability are theoretically unpredictable and practically uncontrollable, at least by states.

Kees van der Pijl treats Locke and Hobbes as empirical representers of a zonal differentiation in world society or at least in regional political culture and political economy. Nationalist or "Hobbesian" patterns of accumulation, beginning in France, spread to Prussia/Germany, Japan, and Italy. The "Lockean heartland" of the world economy, with its "liberal" pattern of accumulation, spread colonially from England to the English-speaking world and then integrated Germany after World War II: the character and security of that integration is the specific focus of his chapter.

Warren Mason's introduction suggests that the *neo* in *neostructuralism* involves a new openness to empirical and positivist methods and an inclination to dialogic, rather than polemical, discourse, with a potential for transideological synthesis. Mason's intended audience should probably read (in order) Frank, Gills, Halperin, Moseley, Chase-Dunn, Palan, and Cerny. Wilde and Lipietz (and then, again, Moseley and Chase-Dunn) may be more appealing to nonmainstreamers interested in the synthetic project. Pijl has constructed a pioneering piece of "creative marginality" that should now be inserted into the dialogue initiated by Fukuyama and by Huntington and his colleagues (*Clash of Civilizations?*, 1993) and taken to a global scale: Can Lockean political-economic culture achieve global reach without giving full employment to Machiavelli, Hobbes, Hegel, Marx, and Nietzsche?

University of California, Los Angeles DAVID WILKINSON

Chinese Foreign Policy: Theory and Practice. Edited by Thomas W. Robinson and David Shambaugh. Oxford: Clarendon, 1994. 644p. $59.00.

The product of a five-day conference in August 1990, this impressive volume has ambitious goals. First, the 21 contributors are asked to examine the People's Republic of China (PRC) since its founding in 1949 to uncover the principal sources and patterns of its foreign policy behavior. Second, the authors are asked "to bridge the long-standing gap between the field of international relations studies and those who, schooled in the area studies tradition, study Chinese foreign policy" (p. 1). The editors take pride in their interdisciplinary approach, inviting chapters from historians, economists, and political scientists within academia, as well as government analysts, Beltway consultants, and former diplomats closer to the action. Contributions from a mainland Chinese political scientist (Wang Jisi) and a leading international relations theorist (James Rosenau) spice the mix. Organizationally, the individual chapters fit neatly into six sections: domestic sources of Chinese foreign policy, international sources of Chinese foreign policy, China's bilateral and regional relationships, patterns of behavior in Chinese foreign policy, international relations theory and the study of Chinese foreign policy, and Robinson's concluding overview attempting to integrate the findings of the other 20 chapters.

Each of the contributors is a well-known specialist, so it is not surprising that the individual chapters are generally very successful, and the volume as a whole is a welcome addition to the field of Chinese foreign policy studies. It should find its way not only onto the shelves of academics and diplomatic practitioners but also onto class syllabi. Indeed, a number of valuable insights emerge from this work. Barry Naughton, for example, intriguingly views China in terms of economic regions and shows how these different regions, in effect, favor different foreign policies. Separate chapters on China's relationships with the United States, the Soviet Union, Europe, Africa, the Middle East, and Asia collectively reveal the relative importance China has attached to these relationships over time and how relations with the two superpowers have influenced China's interactions in Europe and the Third World.

Somewhat more problematic, however, is the avowed attempt to integrate Chinese foreign policy, comparative foreign policy, and international relations theory by opening lines of communication between these scholarly communities. In this regard, it is striking that the more successful integrators tend to be found outside American academia.

Wang Jisi perhaps comes closest to fulfilling the mission of the conference. In what is arguably the strongest chapter in the book, he provides a fine analysis of the evolution of Chinese theories of international relations, as well as Chinese attitudes toward Western theories. Carol Hamrin, a China specialist at the Department of State, likewise views Chinese foreign policy from a theoretical perspective, using concepts from Rosenau and Samuel Kim to build what she calls a developmental theory. By applying Chinese foreign policy shifts to Rosenau's theory of adaptation, empirical generalizations can be hypothesized and investigated comparatively. Hamrin is thus able to suggest in which areas and at what times China's experience has adhered to expectations derived from Rosenau's theory and under what circumstances PRC history provides an exception, suggesting either that the PRC system possesses some unique qualities or that Leninist systems should be treated as a separate category. Yet another chapter that presents a comparative focus is Paul Kreisberg's analysis of Chinese negotiating behavior. The former foreign service officer compares Chinese negotiating style to that of other countries and, based on interviews with diplomats from several nations, shows how Chinese style has evolved and varied depending on a host of factors.

Many of the other chapters refer to international relations theories, some extensively (David Shambaugh's use of "levels of analysis" to analyze Sino-American relations) and others only in passing (as Madelyn Ross does in noting that China's economic behavior has most consistently fit Gilpin's model of nationalism). Among the more successful are Samuel Kim, with his characterization of post-Mao Chinese international organizational behavior as a mixture of neorealism and neofunctionalism, and William Tow, with his use of systems theory to assess China's role in the international strategic system. But there is also a sense, at times, of missed opportunities. For example, Michael Yahuda refers to "perceptions and misperceptions" in Sino-European relations but does not relate his findings to the theoretical writings of such authors as Jervis or Larson. Harry Harding's typically well organized and persuasive chapter on China's cooperative behavior does not address the interdependence literature; a brief discussion of regime theory might have provided a useful introduction to what follows.

James Rosenau's chapter is clearly the most theoretically oriented, and he calls upon "China hands" and specialists on Chinese foreign policy to refine his conceptualization of how the various Chinas he posits might respond to the different global structures derived from such international relations theories as realism, neorealism, pluralism, globalism, Marxism, and his own bifurcationism. His schema, however, is not taken up by other contributors. Indeed, placed at the end of the volume, it serves as a plea to organizers of a future conference, rather than a unifying concept for this volume. The current effort, though stronger on China area expertise than comparative analysis or the integration of international relations theory and area studies, should stimulate subsequent compendia to move still closer to narrowing this divide.

University of Southern California STANLEY ROSEN

Who Adjusts? Domestic Sources of Foreign Economic Policy during the Interwar Years. By Beth A. Simmons. Princeton: Princeton University Press, 1994. 330p. $39.50.

Who Adjusts? is one of the best books I have read in years. Conceiving of international economic cooperation as a problem of credible commitment, Beth Simmons develops a novel, domestic explanation for the policy failures of the Interwar period. Wielding a combination of quantitative tests and case studies, Simmons untangles the complicated web of the interwar international political economy. The book not only sets a new standard of research excellence, but it is required reading for all international political economists, as well as many comparativists and historians.

For over a decade, the study of international cooperation has focused on international regimes and national strategies (e.g., reciprocity) as mechanisms for overcoming dilemmas of common interest and aversion. Recently, attention has turned to the domestic sources of international cooperation. In *Who Adjusts?*, Simmons provides the most compelling treatment to date of this growing research agenda. Central to her analysis—and others of this genre—is the issue of credibility. Even if states want to cooperate, their strategies must be credible both to other states and, where markets are important, to the multitude of individual traders who premise their exchanges on national policies.

In the interwar period, Simmons argues, cooperation meant adherence to the norms of the gold standard, especially those giving priority to external balance over the domestic economy and liberal policies over external controls. The prospects for cooperation were fundamentally weakened after the war by the mobilization of previously disenfranchised segments of the population and the rise of new competition between the major money centers. While these were necessary conditions for the rise of international economic conflict, Simmons suggests, they are not sufficient to explain the failure of cooperation as different countries responded to the changed environment in dramatically different ways.

Simmons develops her central thesis in two steps. She examines, first, the conditions under which credibility is likely to fail, capital to flee, and the current account to deteriorate—in short, when countries will *need* to adjust. On the basis of an aggregate data analysis of 18 countries over 15 years and a detailed case study of France from 1924 to 1927, Simmons finds that external imbalance is more likely when left-wing governments are in power, cabinets are unstable, labor is restless, and central banks are politically controlled. All of these factors affect the market's judgments about the preferences, time horizons, and policy instruments available to policymakers.

Simmons then focuses on the mode of adjustment, specifically whether countries adjust internally (by deflating the economy in line with the norms of the gold standard) or externally (by depreciating the currency or protecting the economy). Using aggregate analysis, France in the 1920s, and additional case studies of Britain, Belgium, and France in the 1930s, Simmons finds that the same factors that predict external imbalance also predict depreciation: unstable, left-wing governments with politically dependent central banks were most likely to go off the gold standard and devalue their currencies. Less willing than others to bear the domestic costs of deflation, such governments are prone to externalize the payments deficit by currency manipulations. The politics of trade policy, on the other hand, are more distinct. Countries that account for a large share of world trade, possess a low degree of trade dependence, and are reliant on customs revenue are most likely to choose protection; moreover, they are likely to raise tariffs during recessions and when conservative or right-wing governments are in power. Simmons concludes that left-wing governments, drawing upon working-class support, are more likely to depreciate their currencies, while right-wing governments, based on the support of capital and capitalists, are more likely to protect their economies.

Together, these findings confirm some of what we knew about the interwar period. More important, however, they suggest that in neither money nor trade did cooperation fail because of the absence of effective international leadership and regimes or the pursuit of beggar-thy-neighbor, cut-throat strategies. Rather, the failures of cooperation lay at home in political bases of support that lacked credibility and led governments to choose their own, independent paths to externalizing their payments deficits.

The strengths of the book are many: its careful grounding of propositions in established theories of economics and politics, its replication of its quantitative results in detailed case studies, and (despite its reliance on aggregate data) its sensitivity to nuance and the contextual nature of politics. The greatest weakness of the book, on the other hand, is its lack of attention to the distributional implications of international economic policy. Simmons makes a persuasive case for domestic macro forces. But, as other recent work shows, even macroeconomic policies (e.g., the exchange rate) have micro effects around which actors will organize for political purposes: depreciations will affect importers differently from exporters, foreign investors differently from noninvestors, fixed asset holders differently from liquid-asset holders, and so on. While international economic conditions may have facilitated the passage of America's infamous Smoot-Hawley Tariff of 1930, for instance, voracious interest groups and the process of congressional log-rolling remain, to my mind, more potent forces than external imbalance. Ignoring such factors produces a stilted and attenuated conception of the politics of interwar economics.

Distributional issues underlie, of course, the left-right policy division Simmons finds central in her cases. But it is at this point, as I read it, that theory becomes most problematic. This division rests on the Stolper-Samuelson theorem, which holds that the abundant (scarce) factor of production will benefit from, and prefer, free trade (protection) (p. 50). Reading backward from the data, this implies that labor was abundant and capital scarce in the countries under analysis. This seems an odd characterization of, say, the United States and United Kingdom, even in the 1930s. Factor proportions, moreover, vary dramatically in the sample of countries, which includes not only these two "leaders" of the international economy but also Bulgaria, Czechoslovakia, and Greece. Unfortunately, the relevant tests for changes in trade policy do not control for national product per capita, a plausible proxy for factor abundance, even though it is included in other equations. Simmons augments this foundation with the argument that left governments are ideologically opposed to consumption taxes that raise the cost of living for the working class. Yet with the expanded franchise after the war, it is hard to see why conservative parties would have eschewed this appeal to the new median voter. Clearly, something else is going on here; but we remain in the dark.

The task before international political economists remains the integration of the macro forces examined by Simmons with the micro forces driven by distributional considerations. There is much that remains to be done. Yet Simmons has pushed macro theories ahead further than others and demonstrated how domestic political factors can be linked both theoretically and practically to patterns of international economic cooperation. It is rare to find such a powerful first book. *Who Adjusts?* will certainly become a classic analysis of interwar economic policy and, more generally, international political economy.

University of California, San Diego DAVID A. LAKE

India's Ad Hoc Arsenal: Direction or Drift in Defence Policy? By Chris Smith. Oxford: Oxford University Press, 1994. 267p. $39.95.

This book is a substantial contribution to filling in the gap on Third World states in the literature on arms industry and military procurement. Its particular merit is that it covers a major state, India, and looks closely at the domestic side of its defence procurement policy. Chapter 2 gives a general overview of regional security in South Asia. Chapters 3–6 are a chronological account of India's defence procurement and policy from 1947 to 1991. Chapter 7 looks at India's attempts at indigenous defence production. Chapter 8 examines nuclear policy and production, and chapter 9 focuses on the bureaucratic politics of Indian defence policymaking.

The core theme is India's failure to define its defence policy in relation to any coherent conceptualization of either its security needs or its economic interests. Within India, there is a central tension between a general aspiration to great-power status (an abiding and widely held notion among both the elites of Delhi and the Indian electorate at large) and a specific set of rivalries with its neighbors, particularly China and Pakistan. Among other things, the great-power aspiration leads to extravagant naval procurements that have little relevance for India's main land-based defence problems, which lie to the north and northwest.

This tension is mediated by the particular qualities of the defence procurement process in India, involving the familiar ingredients of interservice rivalry, cycles of technological obsolescence, corruption, and a political process hampered by lack of information. The result, according to Smith, is a long-running incoherence in India's defence procurement. The great-power aspiration demands broad-spectrum military strength, first-rank technology, and a degree of self-reliance in military production. The need to compete with local rivals supplied with modern weapons by the West, particularly Pakistan, has added to the pressure for first-rank technology, as has a culture of modernity in the Indian military establishment itself. These pressures have worked against the development of indigenous military production in India, which has been mismanaged and has regularly lost out to expensive procurement from the West or the Soviet Union. Soviet supply has been particularly generous; but even that has not resulted in much successful transfer of technology, and, since 1991, is moving away from the soft-currency deals that favored India. The result, Smith argues, is that India has had armed forces that are more expensive and less effective than they could have been. The imported technology is often poorly supported and logistically complex. The conspicuous cultivation of a home-grown nuclear option has, in a sense, covered the holes in India's conventional profile, and enabled the country to bid for recognition of at least regional great-power status. This, however, has come at the cost of putting India outside the increasingly global nuclear nonproliferation regime.

One could quibble with a number of aspects of the book. It is not always as well edited as one would expect from Oxford University Press. There are some rough edges and contradictions. For example, we are told that Pakistan has the most advanced infrastructure in South Asia and that its infrastructure is in an appalling state (pp. 22–23). The author appears to argue that indigenous defence production is, and has been, a real alternative to imports (p. 218); yet throughout the book, the argument seems to be that India could never compete in advanced military technology (esp. pp. 222–23). Some of the historical interpretation could be questioned, and in some places (e.g., India's missile programs) the coverage is too thin. Perhaps the major weakness is that the analysis lacks a comparative element. One can understand the need to constrain the scope of the book, but more could have been done to link the analysis to what is known about other countries. The absence of comparison makes it a bit too easy for the author to take a critical line about the incoherence and inefficiency of India's defence policy and procurement. Would one come to different conclusions if one applied the same criteria to a study of Britain, the United States, South Africa, or Brazil?

Against this, the book provides a complete overview of the Indian case, with a good sense of the internal dynamics shaping India's defence policy and procurement and the ways in which these do and do not line up with the external pressures. It will be particularly valuable for those seeking to understand the domestic dynamics of arms racing in Third World states. Given its critical line, this book is unlikely to be the last word on the subject. But it does give a novel and detailed look at the internal defence policymaking of a major Third World power and offers a challenging interpretation to India's policymakers.

University of Warwick BARRY BUZAN

America's Mission: The United States and the Worldwide Struggle for Democracy in the Twentieth Century. By Tony Smith. Princeton: Princeton University Press, 1994. 453p. $24.95.

Tony Smith's *America's Mission* is an ambitious, provocative, and welcome examination of the United States' century-long effort to export democracy. By focusing on U.S. foreign policy efforts in a sorely neglected area, Smith deftly dismisses the view that the discourse of democracy is mere rhetoric and embeds that effort within the doctrine of Wilsonian liberal internationalism. Wilsonianism, according to Smith, has three tenets:

liberal democracy, free trade, and antimilitarism-multilateralism, a combination that promotes international stability. In making this move, Smith overcomes the sterile realist-idealist divide, adroitly repudiates the realist caricature of Wilsonian thought, and convincingly demonstrates how liberal internationalism represents both an important tradition in American diplomatic practice and a well-grounded and time-honored approach to creating a more stable and desirable international order.

The book's principal focus is an examination of "various attempts by the U.S. to foster democracy abroad in terms of their [*sic*] own ambitions." Smith proceeds thematically and chronologically to examine most major U.S. efforts to export democracy (in the Philippines, Central America, and Japan, and Germany), discusses the character of Wilsonianism, and surveys nearly all American presidents since Franklin Roosevelt. Three conclusions emerge from this survey: (1) the United States frequently spent more time undermining democracies than nurturing them; (2) the United States was reluctant to support those economic policies (e.g., land reforms) that might facilitate democratization if they harmed entrenched economic interests; (3) democracy was also expendable if it threatened the United States' strategic objectives. United States support for democracy, in effect, lasted as long as it did not challenge economic or strategic interests—rare moments, indeed. Although a number of cases are consistent with Wilsonianism, by and large, Smith's conclusions do not uncover any sustained support for the export of democracy, a testimony that his thorough consideration of alternative hypotheses and judicious examination of the evidence.

If the individual cases demonstrate that when strategic and economic imperatives conflict with the support for democracy, the former is apt to prevail, then Smith's overall portrayal of U.S. foreign policy lends strong support for Wilsonianism. Smith is at his best when arguing that Wilsonian liberalism and realism are not competing but, rather complementary visions, suggesting that realism tells us little more than that states pursue security but not how they will do so. Wilsonian liberalism must be understood, he argues, as a means to an end, rather than an end in itself. A Wilsonian sensibility attempts to promote security through assurance, rather than deterrence, reinforced by institutions, rather than balances of power.

Although Smith claims that Wilsonianism is how American leaders play the game of balance-of-power politics (p. 144), this detracts from his rather provocative observation that the conduct of American foreign policy has been organized around two competing visions of how to foster international order: a balance of power scheme, typified by Kissinger and associates, and a Wilsonian worldview. While some U.S. officials championed a balance-of-power approach, many others followed Wilsonian precepts and attempted to instill a higher purpose and ethical content to foreign policy. In this respect, Smith brings to life E. H. Carr's observation that "the homo politicus who pursues nothing but power is as unreal a myth as the homo economicus who pursues nothing but gain" (*The Twenty Year's Crisis* [1964], 97). The conduct of foreign policy is not about the primacy of material or normative forces but, rather, about how their combination unfolds in practice.

These contributions aside, the book contains various problems that undermines its potential impact. As Smith employs the concept of Wilsonianism to evaluate various episodes, he generally drops free trade and multilateralism and focuses solely on democracy, leaving the impression that he is tailoring the concept to suit the subject. For instance, Smith claims that Ronald Reagan was a Wilsonian, because he was committed to exporting democracy. This conceptual elasticity will leave many readers unconvinced. Still, Smith reminds us that Reagan was committed to winning not simply the Cold War but also an ideological war.

Second, there is scant evidence in support of a principal claim that the United States is responsible for the recent surge in democratization. Not only does Smith not provide any systematic examination of the recent wave, but because he focuses on the U.S. policies toward select countries (where the record shows the United States might have done more damage than good), he is unable to consider how the United States (and others) contributed to a particular normative structure in international politics that invested tremendous legitimacy in democratic forms of governance.

Third, there is little sensitivity to issues of case selection or systematic evaluation of the key hypotheses across the cases. I suspect that one reason for the book's methodological weaknesses is its attempt to reach a nonacademic audience. Nevertheless, such shortcomings will not convince most academics of his causal claims. Also absent is a summary statement concerning other key concerns, including under what conditions Wilsonianism is likely to be a force in U.S. foreign policy, when the export of democracy might succeed, or what policies are most likely to produce this outcome. I suspect that the real contribution of the book will be its formulation (rather than confirmation) of a number of hypotheses concerning international politics, the conduct of U.S. foreign policy, and the international sources of democratization.

Notwithstanding these problems, Tony Smith has crafted a compelling and highly welcome account of how Wilsonianism has shaped the conduct of U.S. foreign policy. In so doing, he makes a real contribution to debates on democratization, the current direction of U.S. foreign policy, and international relations theory. This important book deserves a wide audience.

University of Wisconsin MICHAEL N. BARNETT

The War Puzzle. By John A. Vasquez. Cambridge: Cambridge University Press, 1993. 368p. $64.95 cloth, $18.95 paper.

The study of warfare in the past decades has undergone a scientific revolution that is still underway, although this process is already in the third (perhaps the fourth) generation of scholarship. The sea-changes that have occurred in theory and methodology are still in many ways in their descriptive infancy; they are not yet a stable fait accompli. Readers of this journal will know that many political scientists have contributed to advancing our basic understanding of war in the international system. However, so have many scientists working in allied disciplines that also investigate the war phenomenon in human society. The size and variety of this scientific community are large because "the war puzzle" is objectively immense, involving consequential stakes, diverse belligerents, complex causes, and millenarian antiquity. Given these challenging characteristics, the war puzzle ranks in the class of great puzzles of science, along with the basic structure of matter, the evolutionary dynamics of species, the onset of severe weather, or planetary tectonics.

The stated purpose of this book is ambitious and important: to contribute a new understanding of war and peace based on scientific investigations of the past 25 years conducted mostly by political scientists. The book is both a large-scale review of extant research on the causes of war and the conditions of peace (mostly but not exclusively the Correlates of War Project of the University of Michigan, directed by J. David Singer), as well as a new synthesis or explanatory framework to organize the research findings. Building on earlier theoretical perspectives (L. F. Richardson and the Correlates of War Project), the core of the proposed framework to explain the onset and expansion of warfare is founded on the joint causal effects of territoriality, power politics behavior (the "realist tradition"), and alliances. The emphasis of this effort is on explaining a specific class of wars—those that occur among relatively equal belligerent nation-states, a class of phenomena that the author calls "wars of rivalry." Although this omits some major parts of the total war puzzle—civil wars, ethnic warfare, imperial or colonial wars, wars of political disintegration, revolutionary wars, and, most significantly, all the history of warfare in pre-nation-state times (this last category covers approximately 95% of warfare's global timeline)—it must be recognized that all major types of severe warfare are indeed included in the scope of this work. Also, foremost among the class of "wars of rivalry" are all world wars and great-power wars in modern history. As Richardson first remarked in *Statistics of Deadly Quarrels* (1960), in modern history (since A.D. 1815), world wars account for the overwhelming proportion of human fatalities caused by intentional violence (along with criminality), not the more frequent type of midrange warfare involving weak states.

The book has nine chapters and an appendix containing a valuable propositional inventory for testing and developing the proposed framework. The overall organization of the book is logical: the first three chapters are "Preliminaries" (conceptualization of war, typology, and relationship to power politics), while the remaining chapters are "The Onset and Expansion of Wars of Rivalry." Part 2 includes four chapters on warfare proper (territorial causes, escalation, domestic factors, and world wars), one chapter on peace (its properties and relationship to war), and a concluding chapter, "Solving the Puzzle of War." The highly focused nature of this book—warfare among roughly equally powerful modern nation-states, including recent world wars—and the arduous work that is shown by the extensive coverage and incorporation of the extant political science literature make this a highly original and valuable contribution at a critical time when specialization and technical improvements in methodology threaten the unity of the scientific search. The only other recent monographic works that attempt a comparable synthesis in political science—albeit with significantly different goals for the class of warfare phenomena they examine—are perhaps *Peace and War*, by Kalevi J. Holsti (1991), *National Choices and International Processes*, by Zeev Maoz (1990), and *Advancing the Scientific Study of War*, edited by Stuart A. Bremer and Thomas R. Cusack (1994). Perhaps not coincidentally, *The War Puzzle* is published in the same series of Cambridge Studies in International Relations as the first two of these books, a series that is today one of the leading publishers of scientific monographs on world politics. This book is significant, particularly in its contribution to the conceptual integration of research findings that have appeared in disparate professional journals and have rarely been interrelated by the primary investigators. The author's style is eminently readable and accessible to both graduates and motivated undergraduate students. The only requirements are minimal background in empirical research design (mostly Mills's rules) and basic measurement approaches.

The author of this monograph is well known to political scientists for his earlier compilations and assessments of scientific progress in the field of international relations, including *In Search of Theory* (with R. Mansbach, 1981), *The Scientific Study of Peace and War* (with Marie T. Henehan, 1992), and *Classics of International Relations* (1990) but may be less known in allied fields that also study war. The book is addressed primarily to political scientists in the "peace research" tradition: the normative tone in searching for a solution to war is explicit, as in L. F. Richardson's work; but this responsible scientific position never interferes with the author's objective judgment based on the available empirical evidence. Many in economics, anthropology, statistics, and the fledging field of social science history will find this monograph of significant value, both for assessing their own work on warfare and for extending bridges with political science.

The stated aim of this book—to assess and logically integrate into an explanatory framework major portions of the extant empirical knowledge on interstate war onset and expansion—is achieved. This is not to say (as the author himself acknowledges) that the framework is fully developed or validated. Readers will find in this stimulating book more than a rich propositional inventory or an up-to-date review of major portions of the extant literature, valuable as these instrumental accomplishments might be for scientific accumulation. Several features make this contribution particularly valuable: the sustained emphasis on scientific accumulation as essential for understanding war as an empirical phenomenon, the care in the definition of relevant concepts and the effort systematically to integrate an otherwise disorganized mass of disparate empirical findings. These features also provide a motivation for developing more formally articulated theory. For example, the material in chapter 5, "The Realist Road to War," lends itself to mathematical modeling using either conventional extensive form games in forward logic or Brams's newly developed "theory of moves." These are all commendable accomplishments that will add enduring value to this book.

At the same time, some lacunae will dissatisfy some readers. Some of these omissions include the absence of any discussion of findings from areas such as (1) formal analyses of warfare, including significant properties that are derived mathematically, not statistically (e.g., the probabilistic and equilibrium properties of warfare discussed by M. Nicholson in *Formal Theories in International Relations*); (2) the stochastic properties of warfare, including distinct patterns of probabilistic "randomness" that are historically established (discoveries by H. Howeling, M. I. Midlarsky, I. D. Petersen, and L. F. Richardson); (3) some counterintuitive onset-related phenomena, including those related to deterrence and escalation (Q. Wright, J. C. Harsanyi, R. Selten); and (4) scientific knowledge produced by others outside political science (e.g., R. Carneiro's theory of environmental circumscription for explaining warfare and the origin of the state and W. J. Horvath and H. K. Weiss's discovery of warfare's contagious duration, in relation to chaps. 4 and 7, respectively). These are noteworthy omissions in a work aimed at the accumulation of scientific knowledge. Perhaps a future edition could incorporate the new knowledge that has been obtained in these other areas, or some readers will be prompted to explore these connections anyway. In spite of these shortcomings (inevitable and relatively minor deficiencies by comparison with the main accomplishments), this book must be highly recommended to anyone with a scientific inclination to understand warfare. This work constitutes a major addition to an already noteworthy list of monographs in the Cambridge Studies in International Relations.

Finally, the legibility of the text typeface is excellent, as is the binding, and the low number of typographical errors—all indicative of good quality control in a field that has improved its standards for scientific publication. Given the nature of the war puzzle and the broad coverage of this book, I would have preferred a somewhat more extensive reference section (perhaps annotated), a more detailed subject index, a glossary of terms to focus greater attention on the key concepts, and more tables and graphics for summarizing the properties of the war puzzle that have already been deciphered (descriptive indicators on the factual onset and expansion of interstate warfare). However, those that are provided are well prepared, organized, and informative.

This is a book that L. F. Richardson would have read with scientific interest and admiration for the many accomplishments that have taken place in the half-century since his pathbreaking work. It provides a challenging standard for the growth of scientific understanding of war. Research funding agencies should also take note of the high return on their limited investment in this field, and the investigators should be proud. The war puzzle may not be entirely solved; but today we understand many more parts of it than Richardson did or, for that matter, than Thucydides ever dreamed imaginable. *The War Puzzle* bears witness to this scientific progress.

University of Colorado CLAUDIO CIOFFI-REVILLA

Reconstructing Realpolitik. Edited by Frank W. Wayman and Paul F. Deihl. Ann Arbor: University of Michigan Press, 1994. 292p. $59.50 cloth, $22.50 paper.

In their introduction, Frank Wayman and Paul Diehl state that the purpose of this volume is "to break apart, analyze, and then reconsider the realpolitik approach to world politics. At the end, we aim to assess what of value remains in the realpolitik approach, what must be amended or discarded, and what might be needed to replace classic realism" (p. 22). *Realpolitik*, or *realism* (the two terms being used interchangeably throughout the book) is certainly in need of such scrutiny; and the editors and authors are to be congratulated for undertaking the task. However, if this book were just another collection of essays speculating about the strengths and weaknesses of realist thought, there would be little to distinguish it from many others that have grappled with this hoary old beast. It does contain extensive theoretical discussions of various features of realpolitik, many of which offer new perspectives on very old terrain; but what distinguishes this book from countless others is its attempt to subject tenants of realist thought to serious empirical tests.

These tests comprise the core of the book, and nine separate empirical studies are presented. Two of these, the contributions of Brian Pollins and of Frederic Pearson and his colleagues, are more descriptive than analytical, the first dealing with the use of military and economic force by major powers in the twentieth century, and the second, with the incidence of military interventions since World War II. Two other studies examine the possible impact of norms on state behavior, a factor that most realist thinkers heavily discount. Gary Goertz and Paul Diehl consider the degree to which the peaceful creation of new states out of old empires was conditioned by the increasing acceptance of the "right of self-determination" since 1816, while Charles Kegley and Gregory Raymond look at whether war-joining in the nineteenth century was influenced by alliance norms. Two additional studies focus on the role that domestic politics may play in state behavior, a point where realists have been under serious attack as of late. Patrick James and Athanasios Hristoulas examine U.S. decisions to become involved in international crises in the post World War II era and compare the impact of some realist factors with domestic political factors. Building on earlier work, Bruce Bueno de Mesquita and David Lalman compare the so-called "joint democracy" proposition with explanations for war involvement based on balance of power and power transition theories. The remaining three studies examine specific types of behavior that play an important role in realist thought. Russell Leng compares the effectiveness of coercive bargaining tactics with tit-for-tat behavior in 40 international crises; Paul Huth attempts to ascertain the conditions under which major powers will make extended deterrence commitments; and Daniel Jones investigates the circumstances under which states will join the weaker or stronger side in a militarized interstate dispute. Framing all these studies like bookends are excellent introductory and concluding essays by Frank Wayman and Paul Diehl.

Taken as a whole, the studies represent good examples of fine empirical research. It is noteworthy that most of the empirical studies use MLE methods, which may signal some kind of a turning point in the scientific study of conflict. And with few exceptions, the research designs and data sources employed are first-rate. Yet I am left with the impression that these empirical probes have not much penetrated the protective armor of realpolitik. The two most challenging probes—focusing on the importance of international norms and domestic politics, respectively—yield mixed results. And a good realist apologist could easily dodge these thrusts by retreating to the position that realism does not say that these factors are irrelevant, only that they *should be* irrelevant. Realists would be the first to concede that statesmen make mistakes—two big ones being to pay attention to norms and to allow domestic politics to intrude in foreign affairs. That these empirical studies are not more enlightening about the strengths and weaknesses of realpolitik is not essentially the fault of the book's editors or authors. Realist thought is not a coherent, integrated theory of state behavior; it is a collection of descriptive and prescriptive observations and hypotheses loosely tied together by logic. It is a first-class "shape shifter": as such, it can continue to resist definitive tests almost indefinitely, I fear.

In spite of this criticism and others that space prevents me from voicing, this book is a very important contribution to realist literature and literature dealing with the scientific study of conflict. The studies included constitute state-of-the-art research, and the editors do a commendable job of tying them together. And the fundamental goal of the book (i.e., amending and refining grand theory based on empirical tests) lies in the direction that should be followed.

State University of New York, Binghamton STUART A. BREMER

Polar Politics: Creating International Environmental Regimes. Edited by O. R. Young and G. Osherenko. Ithaca: Cornell University Press, 1994. 304p. $38.50 cloth, $16.95 paper.

As you would expect from a book planned, edited and partly written by Oran Young, this one has all the strengths of regime analysis developed over the past 20 years. It is executed with care and clarity, and the research is done with painstaking professionalism. It also has most of the weaknesses. Note that it is not actually about polar politics but only five issues of Arctic political economy, set in a general historical and theoretical framework: (1) the trade in seal furs; (2) polar bear hunting and conservation; (3) the administration of Spitzbergen, a former *terra nullius*, whence Nansen set off for the Pole in the *Fram*; (4) attempts to restrict the use of CFC gases to preserve the earth's ozone layer; and, finally, (5) attempts of the nonregime to deal with the phenomenon of Arctic haze. All good stories, well told.

But the weaknesses still matter. The first comes from the underlying premises that bedevil conventional international relations: the problematic of international relations is war and the preservation of international order; states are the primary threats to order and so are the primary actors; the distribution of power among states measured by their capabilities related to interstate conflict is the independent variable for all outcomes, including those in political economy. Thus, structural power in the broader international political economy sense—of influence over outcomes in the world market economy and society in finance, production, and technology—is overlooked. The power of markets and market players, whether reflected in government policies or not, is discounted. The asymmetry in the structural economic power of large rich states or poor small ones is ignored. The final crowning premise of all this is that "regimes takes the form of constitutional contracts between states" (p. 226). These false premises account for the misfit in the book between the analytical framework and the well-told empirical stories. For example, in the "regime" for protecting polar bears from the hunters, the story suggests that it was the media, not governments, that played the critical role. They were powerful agents in the knowledge structure through which power was exercised over the communication of certain beliefs and ideas, rather than others. Conversely, the media played a negative, or silencing, role in the less glamorous issue of Arctic haze—a puzzle for the scientists but a turnoff for the tabloids (p. 216). Nor do the stories tally with the final premise of the analytical framework that regimes consist of constitutional contracts between states. In the CFC case, the Dupont company effectively exercised a veto power over interstate agreement. Yet the framework automatically excludes structural power exercised by firms. Similarly, the market for seal furs and the fur traders of the City of London, who employed more people than the hunters, were the movers behind British government policy. The stories also stress the role of individuals (pp. 223, 247), though this is largely discounted in much current literature (not, significantly, in France; see Michele Girard's *Les individus dans le relations internationale*, 1994.) In fact, in their conclusions, the editors do come round to finding fault with conventional regime analysis. Bivariate analysis is judged inadequate, and a diagram suggests that structural factors ("context") and "leadership" (not necessarily governmental) actually affect outcomes (p. 247). Some of us have been arguing this for nearly a decade and advocating a much wider approach to the study of international political economy. The time has come, it seems, for some hard new thinking from the veritable army of regime analysts in the United States.

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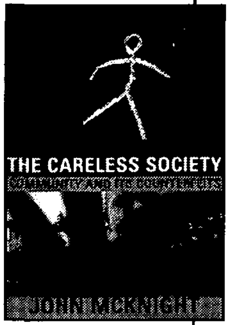

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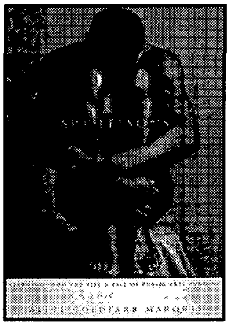

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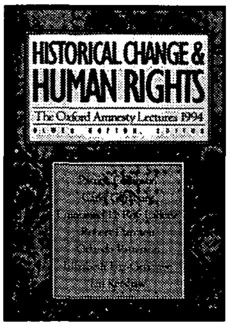
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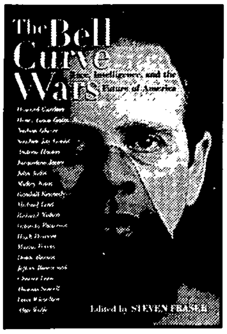

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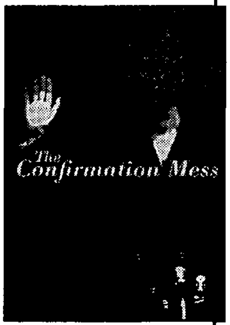

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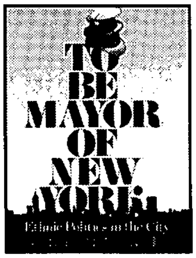

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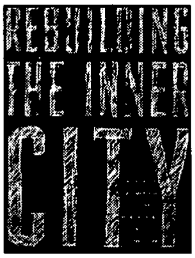

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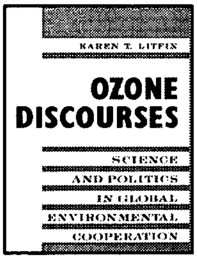

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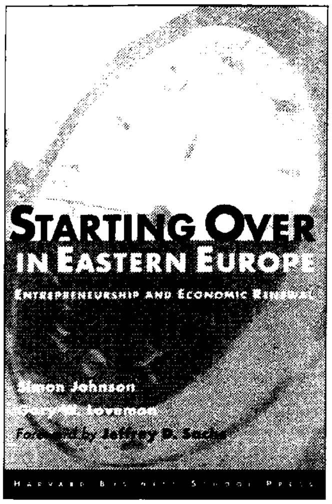
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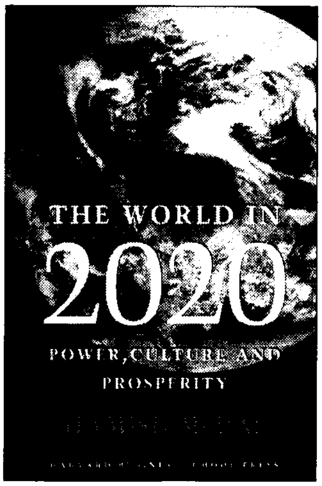
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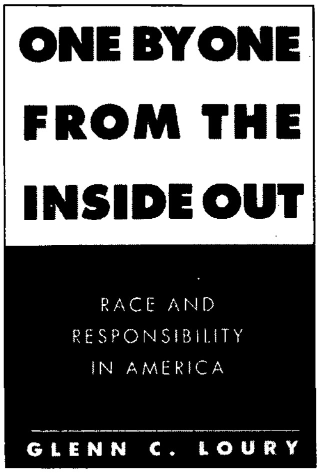

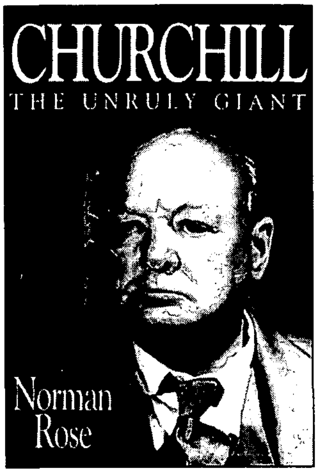

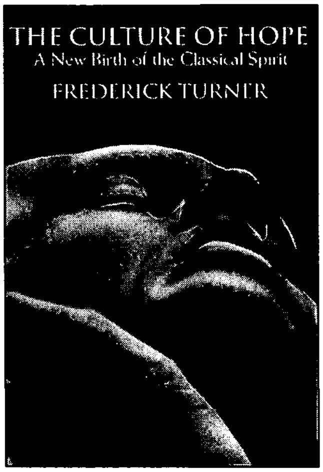

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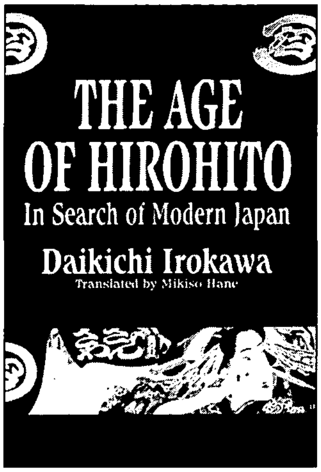

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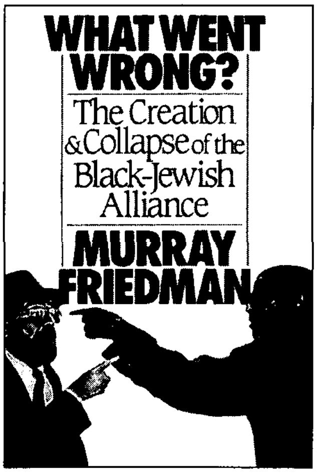

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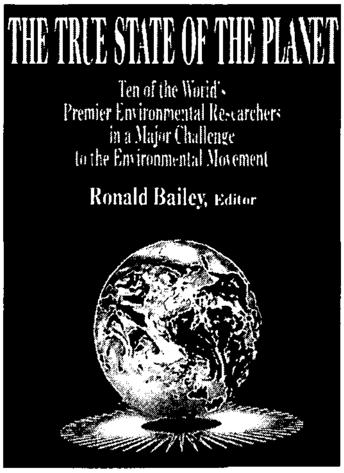
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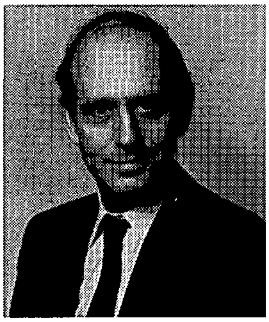

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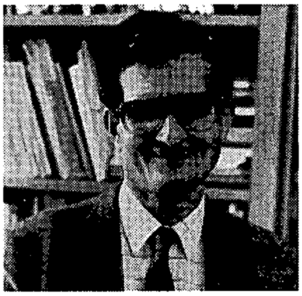

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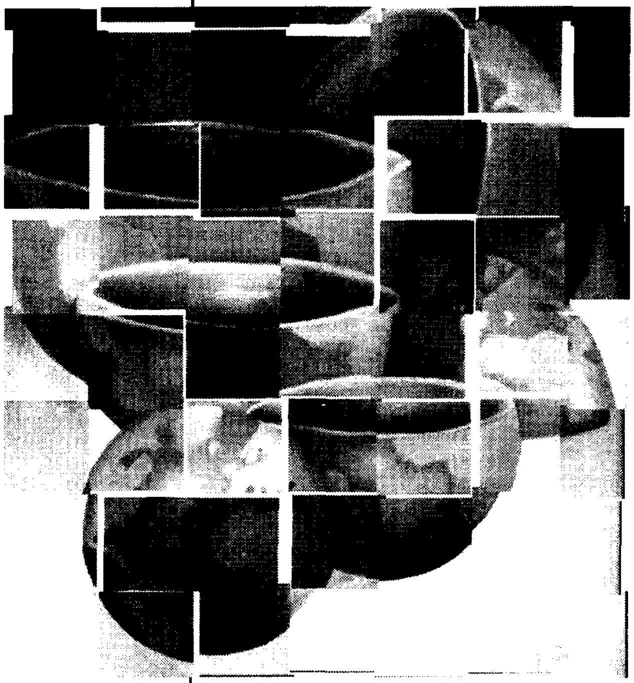

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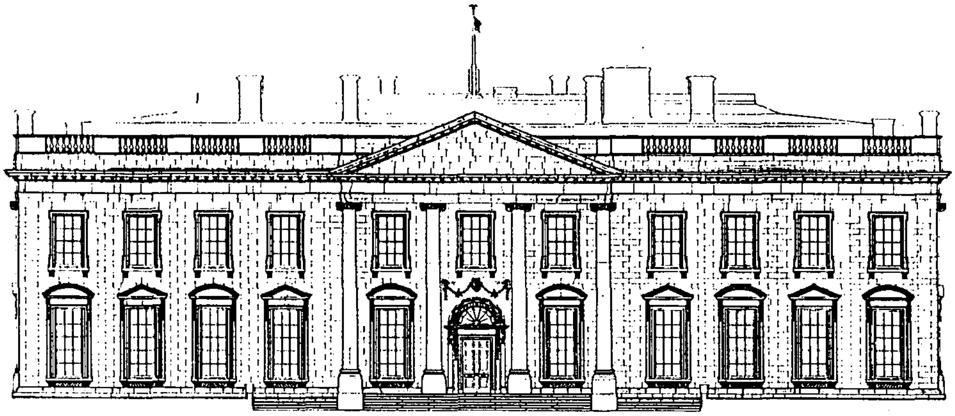

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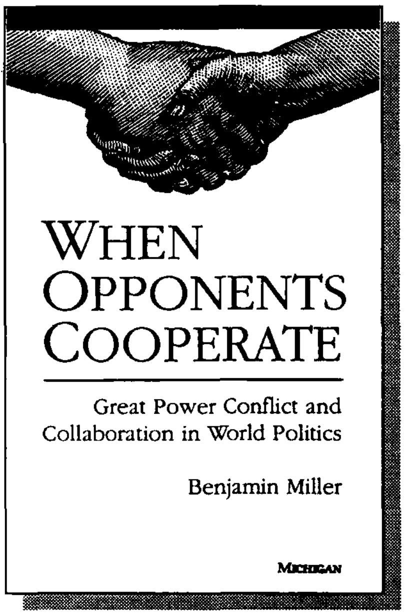

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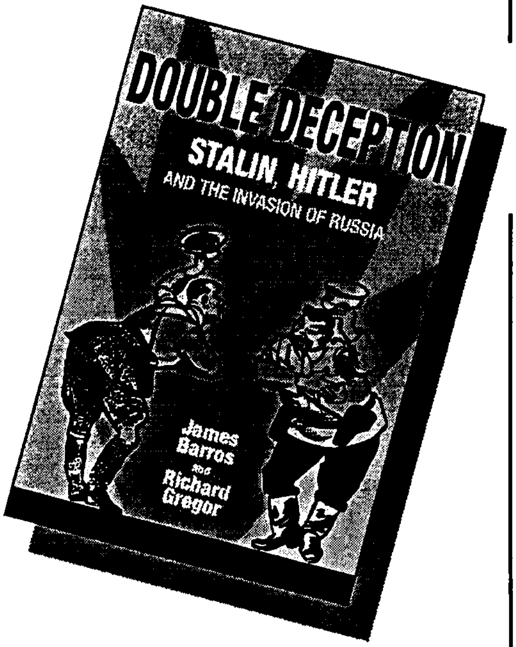

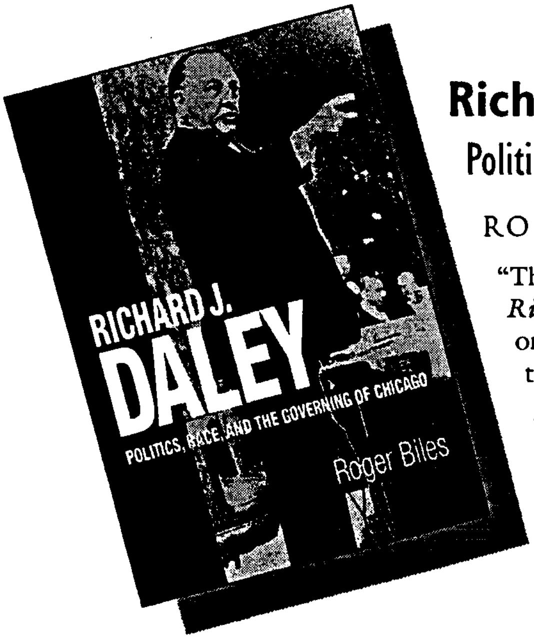

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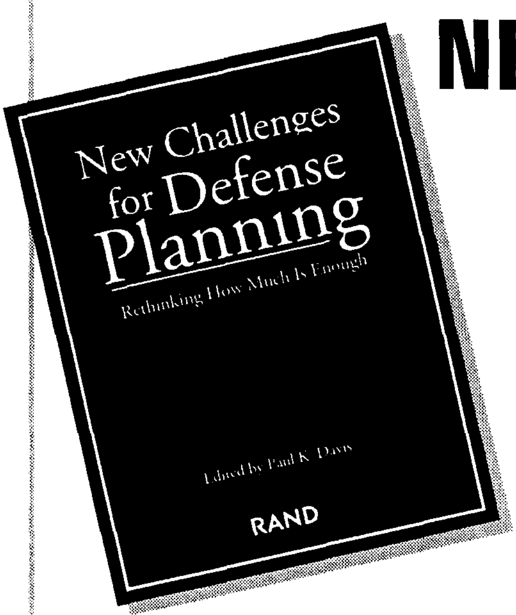
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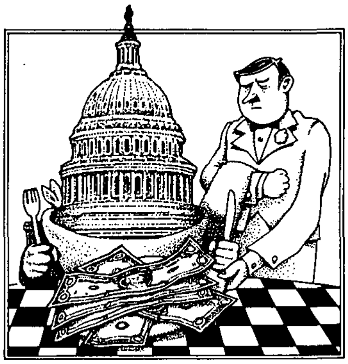

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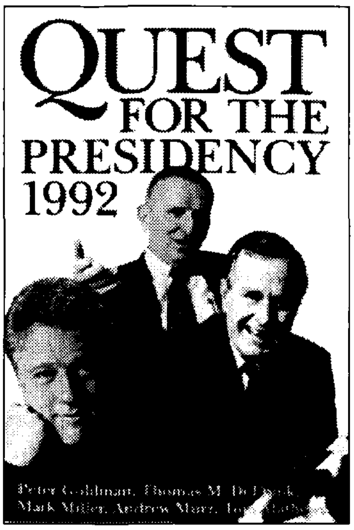
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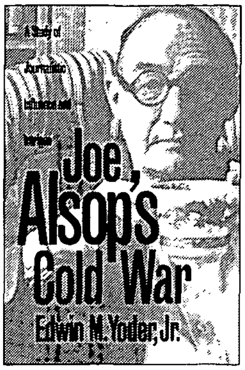
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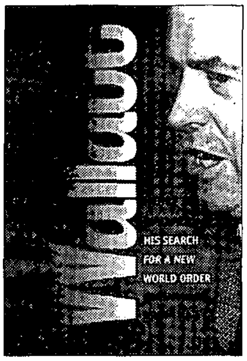
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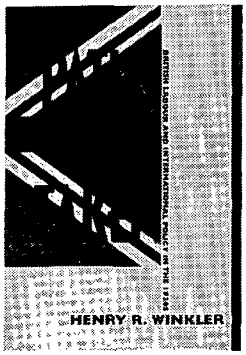
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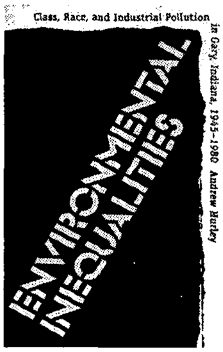
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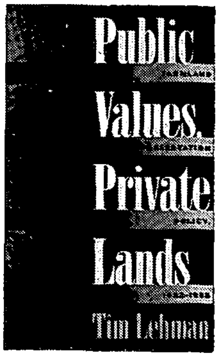
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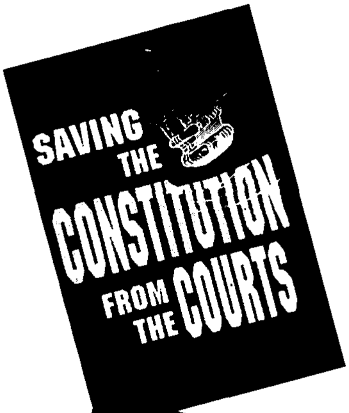

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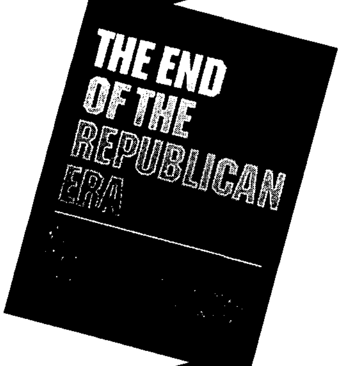

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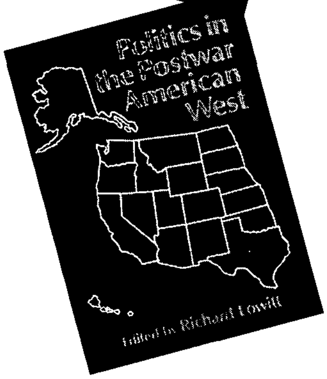

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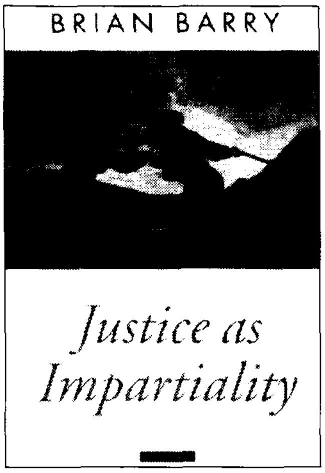

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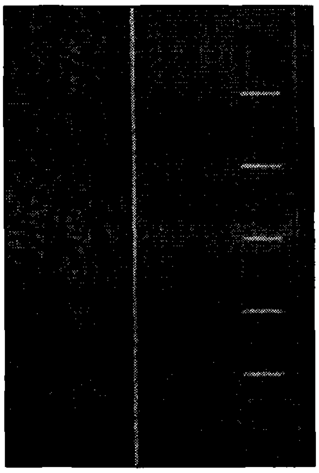

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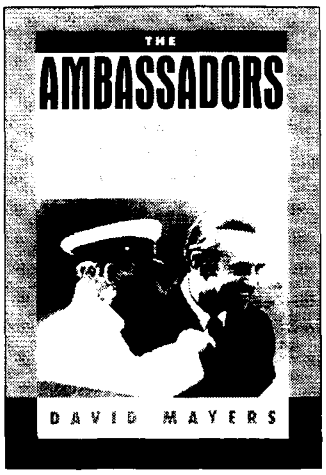

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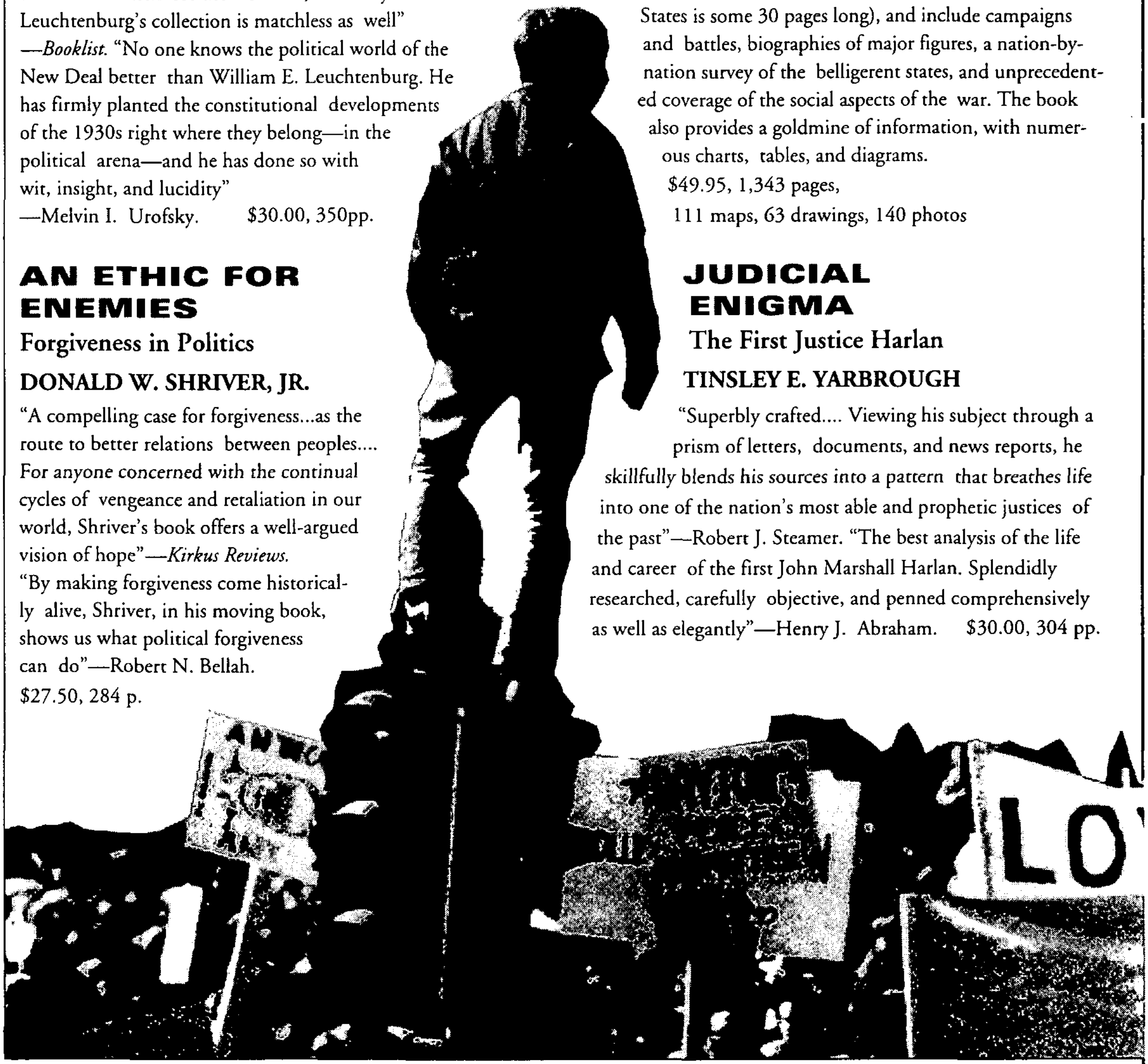

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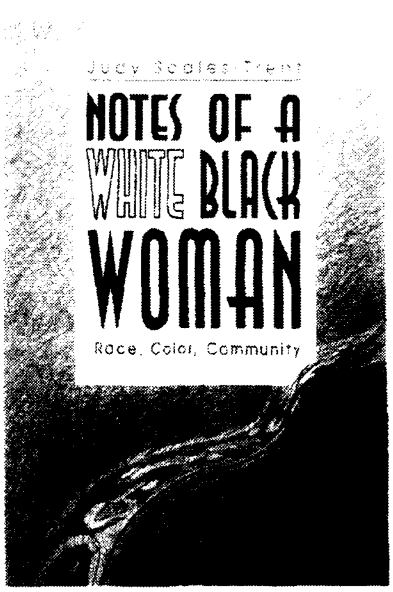

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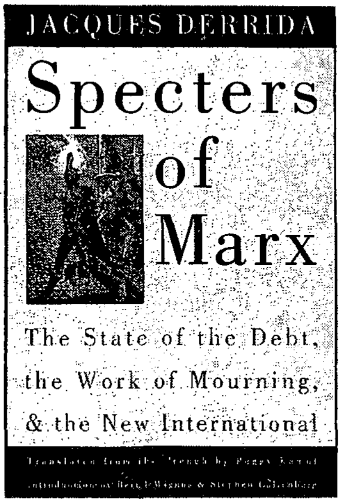
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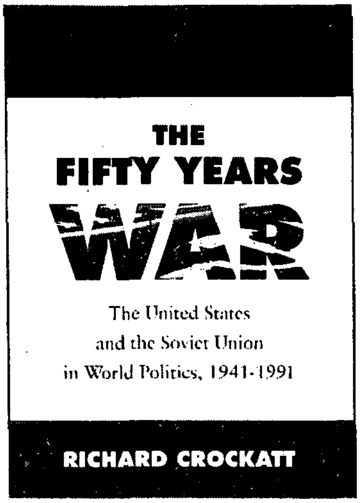
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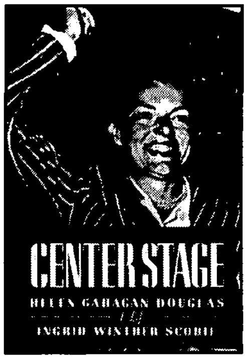

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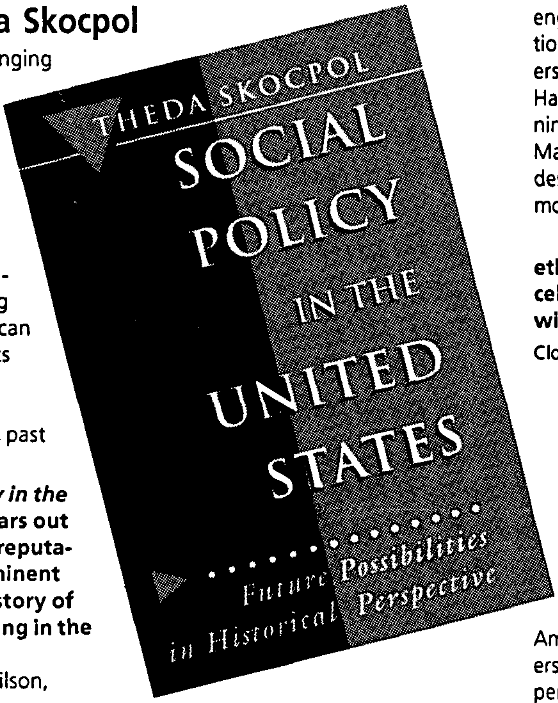
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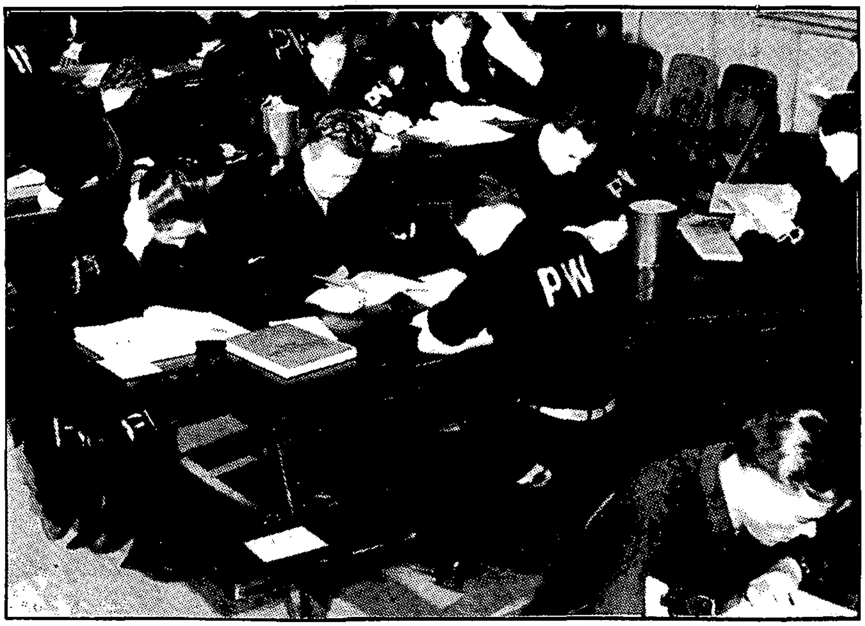

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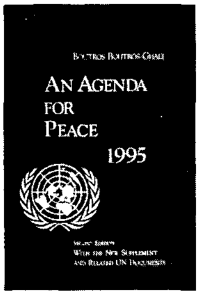
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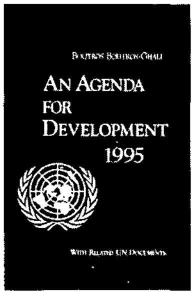
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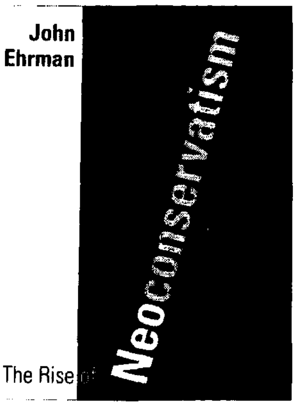

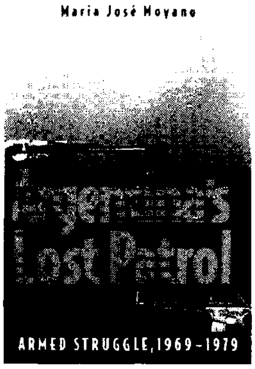

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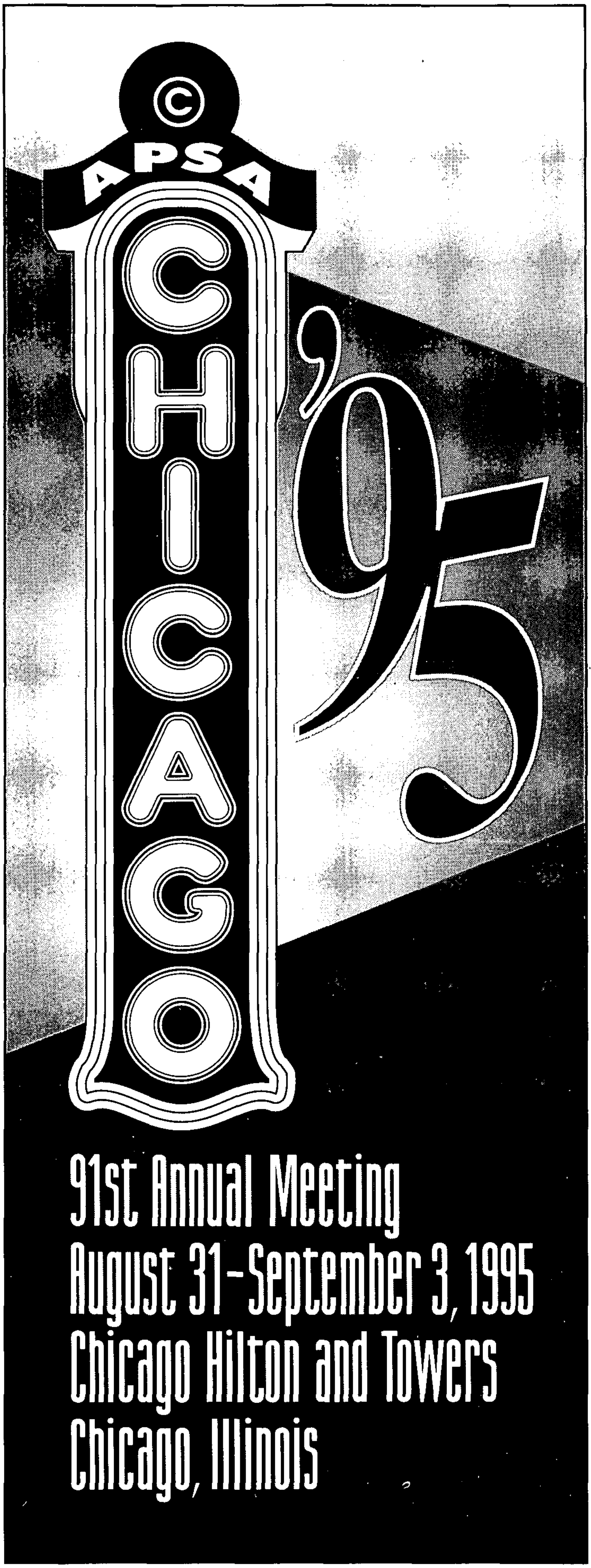

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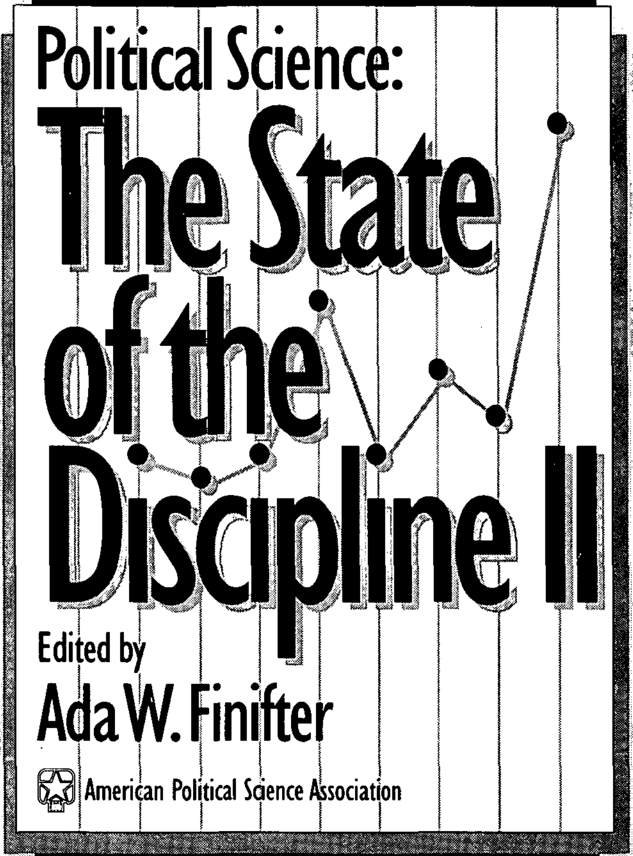

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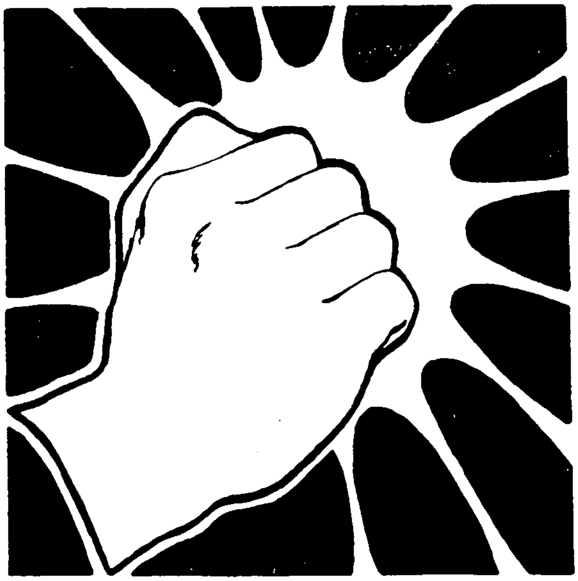

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PURPOSE

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ELIGIBILITY

Competition is open to African American, Latino, and Native American graduates of a baccalaureate institution in the United States **planning to enroll in a doctoral program in the following academic year.** In addition,

- candidates must submit a completed application form with all necessary supporting materials
- applicants must have a record of outstanding academic achievement in political science and other related courses
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Campaign is a revised and expanded version of Frantzich's initial Poli-Ware simulation in which students are randomly assigned to a realistic district and must make a series of strategic campaign decisions with finite resources. Basic principles of campaign strategy are explained and guide the outcome. The simulation is designed to allow the student to write an analytical paper on campaign strategy based on their results after a number of campaigns. ***The American Government Program*** is available for PC computers with a graphics board.

LEARN INTERNATIONAL RELATIONS THROUGH A VIRTUAL WORLD

Chinese House Game is a computer-based simulation is which students make decisions as hypothetical nation-state leaders. Designed for undergraduate International Relations courses by James Lee of American University, the simulations uses a variety of color graphs, maps and tables to provide players with raw materials for making decisions. By playing the game, students gain insight into concepts such as:

- the nature of systems in international relations;
- the process through which states develop;
- the structure of state decision-making;
- the pattern of cooperation and conflict in the atmosphere of competing goals.

The game is played over a number of weeks and brings to life the issues and concepts in course reading and lectures. The emphasis is not necessarily on winning and losing, but rather the learning which comes from interaction in a complex international system. ***Chinese House Game*** is available for PC Computers with a typical configuration.

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All programs may be purchased for $25 per copy (software and user manual) plus $3.50 shipping and handling. Lab Packs, which include 10 copies of the software and one user manual, are available for $149.95. Software in this series has gone through peer review and has been class tested. For further information on the Poli-Ware Program contact Stephen Frantzich, via the American Political Science Association, 1527 New Hampshire Ave., NW, Washington, DC 20036-1290.

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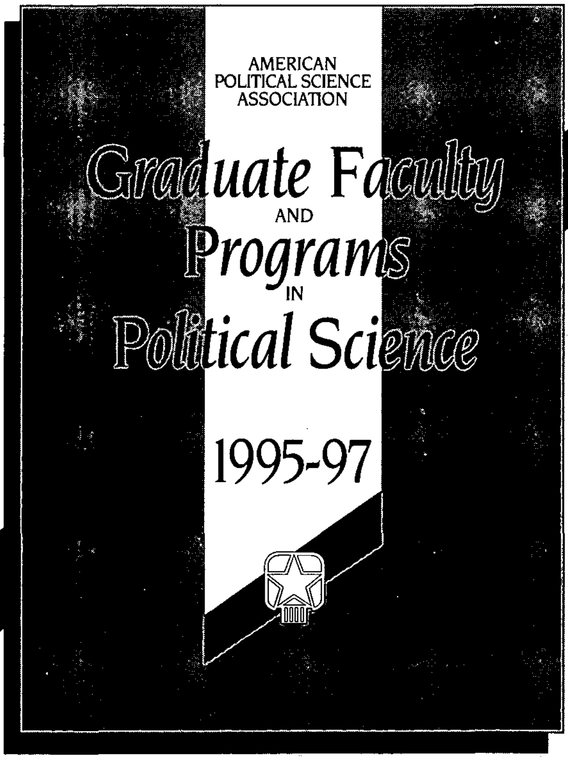

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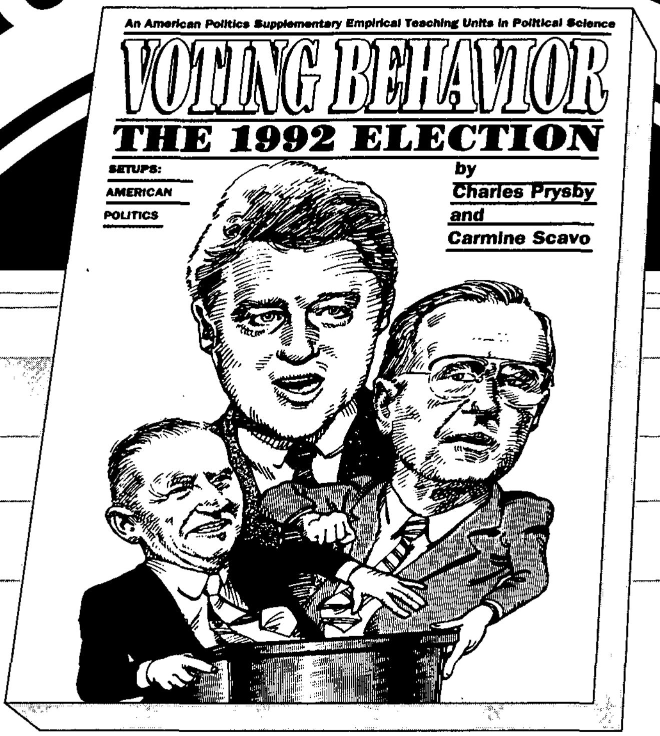

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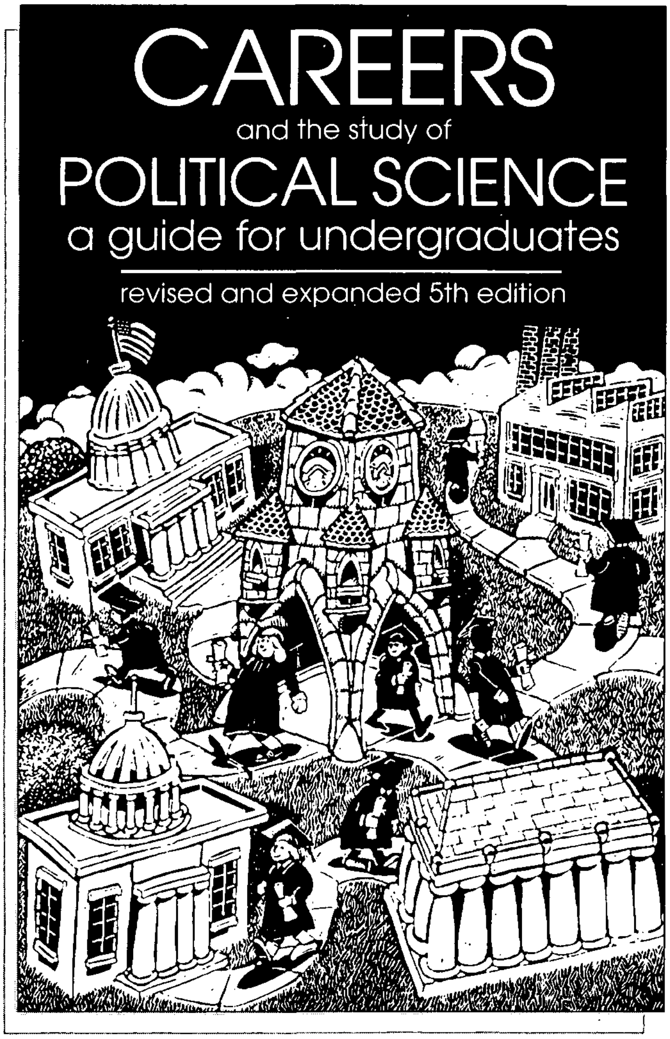

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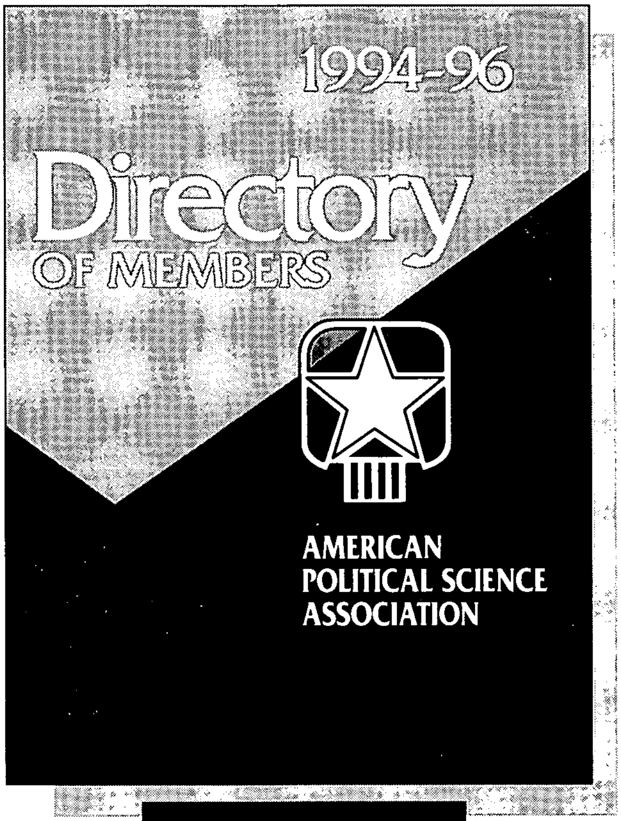

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